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SEQUOIA AND ITS HISTORY.

AN ADDRESS BY PROF. ASA GRAY, PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, DELIVERED AT THE MEETING HELD AT DUBUQUE, IOWA, AUGUST, 1872.

The session being now happily inaugurated, your presiding officer of the last year has only one duty to perform before he surrenders his chair to his successor. If allowed to borrow a simile from the language of my own profession, I might liken the President of this association to a biennial plant. He flourishes for the year in which he comes into existence, and performs his appropriate functions as presiding officer. When the second year comes round he is expected to blossom out in an address and disappear. Each President, as he retires, is naturally expected to contribute something from his own investigations or his own line of study, usually to discuss some particular scientific topic.

Now, although I have cultivated the field of North American Botany, with some assiduity, for more than forty years, have reviewed our vegetable hosts, and assigned to no small number of them their names and their place in the ranks, yet, so far as our own wide country is concerned, I have been to a great extent a closet botanist. Until this summer I had not seen the Mississippi, nor set foot upon a prairie.

To gratify a natural interest, and to gain some title for addressing a body of practical naturalists and explorers, I have made

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a pilgrimage across the continent. I have sought and viewed in their native haunts many a plant and flower which for me had long bloomed unseen, or only in the *hortus siccus*. I have been able to see for myself what species and what forms constitute the main features of the vegetation of each successive region, and record—as the vegetation unerringly does—the permanent characteristics of its climate.

Passing on from the eastern district, marked by its equably distributed rainfall, and therefore naturally forest-clad, I have seen the trees diminish in number, give place to wide prairies, restrict their growth to the borders of streams, and then disappear from the boundless drier plains; have seen grassy plains change into a brown and sere desert — desert in the common sense, but hardly anywhere botanically so; have seen a fair growth of coniferous trees adorning the more favored slopes of a mountain range high enough to compel summer showers; have traversed that broad and bare elevated region shut off on both sides by high mountains from the moisture supplied by either ocean, and longitudinally intersected by sierras which seemingly remain as naked as they were born; and have reached at length the westward slopes of the high mountain barrier which, refreshed by the Pacific, bears the noble forests of the Sierra Nevada and the Coast Range, and among them trees which are the wonder of the world. As I stood in their shade, in the groves of Mariposa and Calaveras, and again under the canopy of the commoner Redwood, raised on columns of such majestic height and ample girth, it occurred to me that I could not do better than to share with you, upon this occasion, some of the thoughts which possessed my mind. In their development they may, perhaps, lead us up to questions of considerable scientific interest.

I shall not detain you with any remarks (which would now be trite) upon the size or longevity of these far-famed Sequoia trees, or of the sugar pines, incense-cedar and firs associated with them, of which even the prodigious bulk of the dominating Sequoia does not sensibly diminish the grandeur. Although no account and no photographic representation of either species of the far-famed Sequoia trees gives any adequate impression of their singular majesty — still less of their beauty — yet my interest in them did not culminate merely or mainly in considerations of their size and age. Other trees, in other parts of the world, may claim to be

older. Certain Australian gum trees (Eucalypti) are said to be taller. Some, we are told, rise so high that they might even cast a flicker of shadow upon the summit of the pyramid of Cheops. Yet the oldest of them doubtless grew from seed which was shed long after the names of the pyramid builders had been forgotten. So far as we can judge from the actual counting of the layers of several trees, no Sequoia now alive can sensibly antedate the Christian era.

Nor was I much impressed with an attraction of man's adding. That the more remarkable of these trees should bear distinguishing appellations seems proper enough: but the tablets of personal names which are affixed to many of them in the most visited groves,—as if the memory of more or less notable people of our day might be made more enduring by the juxtaposition,—do suggest some incongruity. When we consider that a hand's breadth at the circumference of any one of the venerable trunks so placarded has recorded in annual lines the lifetime of the individual thus associated with it, one may question whether the next hand's breadth may not measure the fame of some of the names thus ticketed for adventitious immortality. Whether it be the man or the tree that is honored in the connection, probably either would live as long, in fact and in memory, without it.

One notable thing about these Sequoia trees is their *isolation*. Most of the trees associated with them are of peculiar species, and some of them are nearly as local. Yet every pine, fir, and cypress in California is in some sort familiar, because it has near relatives in other parts of the world. But the redwoods have none. The redwood—including in that name the two species of "big-trees"—belongs to the general cypress family, but is *sui generis*. Thus isolated systematically, and extremely isolated geographically, and so wonderful in size and port, they more than other trees suggest questions.

Were they created thus local and lonely, denizens of California only; one in limited numbers in a few choice spots on the Sierra Nevada, the other along the coast range from the Bay of Monterey to the frontiers of Oregon? Are they veritable Melchizedecs, without pedigree or early relationship, and possibly fated to be without descent?

Or are they now coming upon the stage (or rather were they coming but for man's interference) to play a part in the future?

Or, are they remnants, sole and scanty survivors of a race that has played a grander part in the past, but is now verging to extinction? Have they had a career, and can that career be ascertained or surmised, so that we may at least guess whence they came, and how, and when?

Time was, and not long ago, when such questions as these were regarded as useless and vain,— when students of natural history, unmindful of what the name denotes, were content with a knowledge of things as they now are, but gave little heed as to how they came to be so. Now, such questions are held to be legitimate, and perhaps not wholly unanswerable. It cannot now be said that these trees inhabit their present restricted areas simply because they are there placed in the climate and soil of all the world most congenial to them. These must indeed be congenial, or they would not survive. But when we see how Australian Eucalyptus trees thrive upon the Californian coast, and how these very redwoods flourish upon another continent; how the so-called wild oat (*Avena sterilis* of the Old World) has taken full possession of California; how that cattle and horses, introduced by the Spaniard, have spread as widely and made themselves as much at home on the plains of La Plata as on those of Tartary, and that the cardoon-thistle seeds, and others they brought with them, have multiplied there into numbers probably much exceeding those extant in their native lands; indeed, when we contemplate our own race, and our own particular stock, taking such recent but dominating possession of this New World; when we consider how the indigenous flora of islands generally succumbs to the foreigners which come in the train of man; and that most weeds (*i. e.*, the prepotent plants in open soil) of all temperate climates are not "to the manor born," but are self-invited intruders; — we must needs abandon the notion of any primordial and absolute adaptation of plants and animals to their habitats, which may stand in lieu of explanation, and so preclude our inquiring any further. The harmony of Nature and its admirable perfection need not be regarded as inflexible and changeless. Nor need Nature be likened to a statue, or a cast in rigid bronze, but rather to an organism, with play and adaptability of parts, and life and even soul informing the whole. Under the former view Nature would be "the faultless monster which the world ne'er saw," but inscrutable as the Sphinx, whom it were vain, or worse, to question of the whence and whither.

Under the other, the perfection of nature, if relative, is multifarious and ever renewed; and much that is enigmatical now may find explanation in some record of the past.

That the two species of redwood we are contemplating originated as they are and where they are, and for the part they are now playing, is, to say the least, not a scientific supposition, nor in any sense a probable one. Nor is it more likely that they are destined to play a conspicuous part in the future, or that they would have done so, even if the Indian's fires and the white man's axe had spared them. The redwood of the coast (*Sequoia sempervirens*) had the stronger hold upon existence, forming as it did large forests throughout a narrow belt about three hundred miles in length, and being so tenacious of life that every large stump sprouts into a copse. But it does not pass the Bay of Monterey, nor cross the line of Oregon, although so grandly developed not far below it. The more remarkable *Sequoia gigantea* of the Sierra exists in numbers so limited that the separate groves may be reckoned upon the fingers, and the trees of most of them have been counted, except near their southern limit, where they are said to be more copious. A species limited in individuals holds its existence by a precarious tenure; and this has a foothold only in a few sheltered spots, of a happy mean in temperature and locally favored with moisture in summer. Even there, for some reason or other, the pines with which they are associated (*Pinus Lambertiana* and *P. ponderosa*), the firs (*Abies grandis* and *A. amabilis*) and even the incense-cedar (*Libocedrus decurrens*) possess a great advantage, and, though they strive in vain to emulate their size, wholly overpower the Sequoias in numbers. "To him that hath shall be given." The force of numbers eventually wins. At least in the commonly visited groves *Sequoia gigantea* is invested in its last stronghold, can neither advance into more exposed positions above, nor fall back into drier and barer ground below, nor hold its own in the long run where it is, under present conditions; and a little further drying of the climate, which must once have been much moister than now, would precipitate its doom. Whatever the individual longevity, certain if not speedy is the decline of a race in which a high death-rate afflicts the young. Seedlings of the big trees occur not rarely, indeed, but in meagre proportion to those of associated trees; and small indeed is the chance that any of these will attain to "the days of the years of their fathers."

"Few and evil" are the days of all the forest likely to be, while man, both barbarian and civilized, torments them with fires, fatal at once to seedlings, and at length to the aged also. The forests of California, proud as the State may be of them, are already too scanty and insufficient for her uses. Two lines, such as may be drawn with one sweep of a small brush over the map, would cover them all. The coast redwood,— the most important tree in California,— although a million times more numerous than its relative of the Sierra, is too good to live long. Such is its value for lumber and its accessibility, that, judging the future by the past, it is not likely, in its primeval growth, to outlast its rarer fellow-species.

Happily man preserves and disseminates as well as destroys. The species will probably be indefinitely preserved to science, and for ornamental and other uses, in its own and other lands; and the more remarkable individuals of the present day are likely to be sedulously cared for, all the more so as they become scarce.

Our third question remains to be answered: Have these famous Sequoias played in former times and upon a larger stage a more imposing part, of which the present is but the epilogue? We cannot gaze high up the huge and venerable trunks, which one crosses the continent to behold, without wishing that these patriarchs of the grove were able, like the long-lived antediluvians of scripture, to hand down to us, through a few generations, the traditions of centuries, and so tell us somewhat of the history of their race. Fifteen hundred annual layers have been counted, or satisfactorily made out, upon one or two fallen trunks. It is probable that close to the heart of some of the living trees may be found the circle that records the year of our Saviour's nativity. A few generations of such trees might carry the history a long way back. But the ground they stand upon, and the marks of very recent geological change and vicissitude in the region around, testify that not very many such generations can have flourished just there, at least in an unbroken series. When their site was covered by glaciers, these Sequoias must have occupied other stations, if, as there is reason to believe, they then existed in the land.

I have said that the redwoods have no near relatives in the country of their abode, and none of their genus anywhere else. Perhaps something may be learned of their genealogy by inquiring of such relatives as they have. There are only two of any partic-

ular nearness of kin; and they are far away. One is the bald cypress, our southern cypress, Taxodium, inhabiting the swamps of the Atlantic coast from Maryland to Texas, thence extending into Mexico. It is well known as one of the largest trees of our Atlantic forest-district, and, although it never (except perhaps in Mexico, and in rare instances) attains the portliness of its western relatives, yet it may equal them in longevity. The other relative is Glyptostrobus, a sort of modified Taxodium, being about as much like our bald cypress as one species of redwood is like the other.

Now species of the same type, especially when few, and the type peculiar, are, in a general way, associated geographically, *i. e.*, inhabit the same country, or (in a large sense) the same region. Where it is not so, where near relatives are separated, there is usually something to be explained. Here is an instance. These four trees, sole representatives of their tribe, dwell almost in three separate quarters of the world: the two redwoods in California, the bald cypress in Atlantic North America, its near relative, Glyptostrobus, in China.

It was not always so. In the tertiary period, the geological botanists assure us, our own very Taxodium, or bald cypress, and a Glyptostrobus, exceedingly like the present Chinese tree, and more than one Sequoia, co-existed in a fourth quarter of the globe, *viz.*, in Europe! This brings up the question: Is it possible to bridge over these four wide intervals of space and the much vaster interval of time, so as to bring these extraordinarily separated relatives into connection. The evidence which may be brought to bear upon this question is various and widely scattered. I bespeak your patience while I endeavor to bring together, in an abstract, the most important points of it.

Some interesting facts may come out by comparing generally the botany of the three remote regions, each of which is the sole home of one of these three genera, *i. e.*, Sequoia in California, Taxodium in the Atlantic United States, and Glyptostrobus in China, which compose the whole of the peculiar tribe under consideration.

Note then, first, that there is another set of three or four peculiar trees, in this case of the yew family, which has just the same peculiar distribution, and which therefore may have the same explanation, whatever that explanation be. The genus Torreya, which commemorates our botanical Nestor and a former president of

this association, Dr. Torrey, was founded upon a tree rather lately discovered (that is, about thirty-five years ago) in northern Florida. It is a noble, yew-like tree, and very local, being known only for a few miles along the shores of a single river. It seems as if it had somehow been crowded down out of the Alleghanies into its present limited southern quarters; for in cultivation it evinces a northern hardiness. Now another species of Torreya is a characteristic tree of Japan; and the same, or one very like it indeed, inhabits the Himalayas, — belongs, therefore, to the Eastern Asiatic temperate region, of which China is a part, and Japan, as we shall see, the portion most interesting to us. There is only one more species of Torreya, and that is a companion of the redwoods in California. It is the tree locally known under the name of the California nutmeg. In this case the three are near brethren, species of the same genus, known nowhere else than in these three habitats.

Moreover, the Torreya of Florida has growing with it a yew tree; and the trees of that grove are the only yew trees of Eastern America; for the yew of our northern woods is a decumbent shrub. The only other yew trees in America grow with the redwoods and the other Torreya in California, and more plentifully farther north, in Oregon. A yew tree equally accompanies the Torreya of Japan and the Himalayas, and this is apparently the same as the common yew of Europe.

So we have three groups of trees of the great coniferous order which agree in this peculiar geographical distribution; the redwoods and their relatives, which differ widely enough to be termed a different genus in each region; the Torreyas, more nearly akin, merely a different species in each region; the yews, perhaps all of the same species, perhaps not quite that, for opinions differ and can hardly be brought to any decisive test. The yews of the Old World, from Japan to Western Europe, are considered the same; the very local one in Florida is slightly different; that of California and Oregon differs a very little more; but all of them are within the limits of variation of many a species. However that may be, it appears to me that these several instances all raise the same question, only with a different degree of emphasis, and, if to be explained at all, will have the same kind of explanation. But the value of the explanation will be in proportion to the number of facts it will explain.

Continuing the comparison between the three regions with which

we are concerned, we note that each has its own species of pines, firs, larches, etc., and of a few deciduous-leaved trees, such as oaks and maples; all of which have no peculiar significance for the present purpose, because they are of genera which are common all round the northern hemisphere. Leaving these out of view, the noticeable point is that the vegetation of California is most strikingly unlike that of the Atlantic United States. They possess some plants, and some peculiarly American plants in common,—enough to show, as I imagine, that the difficulty was not in the getting from the one district to the other, or into both from a common source, but in abiding there. The primordially unbroken forest of Atlantic North America, nourished by rainfall distributed throughout the year, is widely separated from the western region of sparse and discontinuous tree-belts of the same latitude on the western side of the continent, where summer rain is wanting or nearly so, by immense treeless plains and plateaux of more or less aridity, traversed by longitudinal mountain ranges of a similar character. Their nearest approach is at the north, in the latitude of Lake Superior, where, on a more rainy line, trees of the Atlantic forest and that of Oregon may be said to interchange. The change of species and of the aspect of vegetation in crossing, say on the forty-seventh parallel, is slight in comparison with that on the thirty-seventh or near it. Confining our attention to the lower latitude, and under the exceptions already specially noted, we may say that almost every characteristic form in the vegetation of the Atlantic States is wanting in California, and the characteristic plants and trees of California are wanting here.

California has no Magnolia nor tulip trees, nor star-anise tree; no so-called Papaw (Asimina); no barberry of the common single-leaved sort; no Podophyllum or other of the peculiar associated genera; no Nelumbo nor white water-lily; no prickly ash nor sumach; no loblolly-bay nor Stuartia; no basswood nor linden trees; neither locust, honey-locust, coffee trees (Gymnocladus) nor yellow-wood (Cladrastis); nothing answering to Hydrangea or witch-hazel, to gum-trees (Nyssa and Liquidambar), Viburnum or Diervilla; it has few asters and golden-rods; no lobelias; no huckleberries and hardly any blueberries; no Epigæa, charm of our earliest eastern spring, tempering an icy April wind with a delicious wild fragrance; no Kalmia nor Clethra, nor holly, nor persimmon; no Catalpa tree, nor trumpet-creeper (Tecoma); nothing answering to

sassafras, nor to benzoin tree, nor to hickory; neither mulberry nor elm; no beech, true chestnut, hornbeam, nor ironwood, nor a proper birch tree; and the enumeration might be continued very much further by naming herbaceous plants and others familiar only to botanists.

In their place California is filled with plants of other types, trees, shrubs and herbs, of which I will only remark that they are, with one or two exceptions, as different from the plants of the eastern Asiatic region with which we are concerned (Japan, China and Mandchuria), as they are from those of Atlantic North America. Their near relatives, when they have any in other lands, are mostly southward, on the Mexican plateau, or many as far south as Chili. The same may be said of the plants of the intervening great plains, except that northward and in the subsaline vegetation there are some close alliances with the flora of the steppes of Siberia. And along the crests of high mountain ranges the arctic-alpine flora has sent southward more or less numerous representatives through the whole length of the country.

If we now compare, as to their flora generally, the Atlantic United States with Japan, Mandchuria, and Northern China, *i. e.*, Eastern North America with Eastern North Asia — half the earth's circumference apart — we find an astonishing similarity. The larger part of the genera of our own region, which I have enumerated as wanting in California, are present in Japan or Mandchuria, along with many other peculiar plants, divided between the two. There are plants enough of the one region which have no representatives in the other. There are types which appear to have reached the Atlantic States from the south; and there is a larger infusion of subtropical Asiatic types into temperate China and Japan; among these there is no relationship between the two countries to speak of. There are also, as I have already said, no small number of genera and some species which, being common all round or partly round the northern temperate zone, have no special significance because of their occurrence in these two antipodal floras, although they have testimony to bear upon the general question of geographical distribution. The point to be remarked is, that many or even most of the genera and species which are peculiar to North America as compared with Europe, and largely peculiar to Atlantic North America as compared with the Californian region, are also represented in Japan and Mandchuria, either

by identical or by closely similar forms! The same rule holds on a more northward line, although not so strikingly. If we compare the plants, say of New England and Pennsylvania (lat. 45°–47°), with those of Oregon, and then with those of northeastern Asia, we shall find many of our own curiously repeated in the latter, while only a small number of them can be traced along the route even so far as the western slope of the Rocky Mountains. And these repetitions of East American types in Japan and neighboring districts are in all degrees of likeness. Sometimes the one is undistinguishable from the other; sometimes there is a difference of aspect, but hardly of tangible character; sometimes the two would be termed marked varieties if they grew naturally in the same forest or in the same region; sometimes they are what the botanist calls representative species, the one answering closely to the other, but with some differences regarded as specific; sometimes the two are merely of the same genus, or not quite that, but of a single or very few species in each country; when the point which interests us is, that this peculiar limited type should occur in two antipodal places, and nowhere else.

It would be tedious, and except to botanists abstruse, to enumerate instances; yet the whole strength of the case depends upon the number of such instances. I propose, therefore, if the Association does me the honor to print this discourse, to append in a note, a list of the more remarkable ones. But I would here mention two or three cases as specimens.

Our *Rhus Toxicodendron*, or poison ivy, is very exactly repeated in Japan, but is found in no other part of the world, although a species much like it abounds in California. Our other poisonous *Rhus* (*R. venenata*), commonly called poison dogwood, is in no way represented in Western America, but has so close an analogue in Japan that the two were taken for the same by Thunberg and Linnæus, who called them both *R. Vernix*.

Our northern fox-grape, *Vitis Labrusca*, is wholly confined to the Atlantic States, except that it reappears in Japan and that region.

The original Wistaria is a woody leguminous climber with showy blossoms, native to the Middle Atlantic States; the other species, which we so much prize in cultivation, *W. Sinensis* is from China, as its name denotes, or perhaps only from Japan, where it is certainly indigenous.

Our yellow-wood (Cladrastis) inhabits a very limited district on the western slope of the Alleghanies. Its only and very near relative, Maackia, is in Mandchuria.

The Hydrangeas have some species in our Alleghany region; all the rest belong to the Chino-Japanese region and its continuation westward. The same may be said of Philadelphus, except that there are one or two mostly very similar species in California and Oregon.

Our blue cohosh (Caulophyllum) is confined to the woods of the Atlantic States, but has lately been discovered in Japan. A peculiar relative of it, Diphylleia, confined to the higher Alleghanies, is also repeated in Japan, with a slight difference, so that it may barely be distinguished as another species. Another relative is our twin-leaf (Jeffersonia) of the Alleghany region alone; a second species has lately turned up in Mandchuria. A relative of this is Podophyllum, our mandrake, a common inhabitant of the Atlantic United States, but found nowhere else. There is one other species of it, and that is in the Himalayas. Here are four most peculiar genera of one family, each of a single species in the Atlantic United States, which are duplicated on the other side of the world, either in identical or almost identical species, or in an analogous species, while nothing else of the kind is known in any other part of the world.

I ought not to omit ginseng, the root so prized by the Chinese, which they obtained from their northern provinces and Mandchuria, and which is now known to inhabit Corea and Northern Japan. The Jesuit Fathers identified the plant in Canada and the Atlantic States, brought over the Chinese name by which we know it, and established the trade in it, which was for many years most profitable. The exportation of ginseng to China probably has not yet entirely ceased. Whether the Asiatic and the Atlantic American ginsengs are exactly of the same species or not is somewhat uncertain, but they are hardly, if at all, distinguishable.

There is a shrub, Elliottia, which is so rare and local that it is known only at two stations on the Savannah River, in Georgia. It is of peculiar structure, and was without near relative until one was lately discovered in Japan (Tripetaleia), so like it as hardly to be distinguishable except by having the parts of the blossom in threes instead of fours, — a difference which is not uncommon in the same genus, or even in the same species.

Suppose Elliottia had happened to be collected only once, a good while ago, and all knowledge of the limited and obscure locality were lost; and meanwhile the Japanese form came to be known. Such a case would be parallel with an actual one. A specimen of a peculiar plant (*Shortia galacifolia*) was detected in the herbarium of the elder Michaux, who collected it (as his autograph ticket shows) somewhere in the high Alleghany Mountains, more than eighty years ago. No one has seen the living plant since or knows where to find it, if haply it still flourishes in some secluded spot. At length it is found in Japan; and I had the satisfaction of making the identification.* One other relative is also known in Japan; and another, still unpublished, has just been detected in Thibet.

Whether the Japanese and the Alleghanian plants are exactly the same or not, it needs complete specimens of the two to settle. So far as we know they are just alike, and even if some difference were discerned between them, it would not appreciably alter the question as to how such a result came to pass. Each and every one of the analogous cases I have been detailing — and very many more could be mentioned — raises the same question, and would be satisfied with the same answer.

These singular relations attracted my curiosity early in the course of my botanical studies, when comparatively few of them were known, and my serious attention in later years, when I had numerous and new Japanese plants to study in the collections made (by Messrs. Williams and Morrow) during Commodore Perry's visit in 1853, and, especially, by Mr. Charles Wright, in Commodore Rodgers' expedition in 1855. I then discussed this subject somewhat fully, and tabulated the facts within my reach.†

This was before Heer had developed the rich fossil botany of the Arctic zone, before the immense antiquity of existing species of plants was recognized, and before the publication of Darwin's now famous volume on the "Origin of Species" had introduced and familiarized the scientific world with those now current ideas respecting the history and vicissitudes of species with which I attempted to deal in a moderate and feeble way.

My speculation was based upon the former glaciation of the northern temperate zone, and the inference of a warmer period

* Amer. Jour. Science, 1867, p. 402; Proceed. Amer. Acad., 8. p. 244.
† Mem. Amer. Acad. vol 6.

preceding and perhaps following. I considered that our own present vegetation, or its proximate ancestry, must have occupied the arctic and subarctic regions in pliocene times, and that it had been gradually pushed southward as the temperature lowered and the glaciation advanced, even beyond its present habitation; that plants of the same stock and kindred, probably ranging round the arctic zone as the present arctic species do, made their forced migration southward upon widely different longitudes, and receded more or less as the climate grew warmer; that the general difference of climate which marks the eastern and the western sides of the continents,— the one extreme, the other mean — was doubtless even then established, so that the same species and the same sorts of species would be likely to secure and retain foothold in the similar climates of Japan and the Atlantic United States, but not in intermediate regions of different distribution of heat and moisture; so that different species of the same genus, as in Torreya, or different genera of the same group, as redwood, Taxodium and Glyptostrobus, or different associations of forest trees, might establish themselves each in the region best suited to its particular requirements, while they would fail to do so in any other. These views implied that the sources of our actual vegetation and the explanation of these peculiarities were to be sought in, and presupposed, an ancestry in pliocene or still earlier times, occupying the high northern regions. And it was thought that the occurrence of peculiarly North American genera in Europe in the tertiary period (such as Taxodium, Carya, Liquidambar, Sassafras, Negundo, etc.), might be best explained on the assumption of early interchange and diffusion through North Asia, rather than by that of the fabled Atlantis.

The hypothesis supposed a gradual modification of species in different directions under altering conditions, at least to the extent of producing varieties, subspecies and representative species, as they may be variously regarded; likewise the single and local origination of each type, which is now almost universally taken for granted.

The remarkable facts in regard to the Eastern American and Asiatic floras which these speculations were to explain, have since increased in number, more especially through the admirable collections of Dr. Maximowitz in Japan and adjacent countries, and the critical comparisons he has made and is still engaged upon.

I am bound to state that, in a recent general work * by a distinguished European botanist, Prof. Grisebach of Göttingen, these facts have been emptied of all special significance, and the relations between the Japanese and the Atlantic United States floras declared to be no more intimate than might be expected from the situation, climate, and present opportunity of interchange. This extraordinary conclusion is reached by regarding as distinct species all the plants common to both countries between which any differences have been discerned, although such differences would probably count for little if the two inhabited the same country, thus transferring many of my list of identical to that of representative species; and, then, by simply eliminating from consideration the whole array of representative species, *i. e.*, all cases in which the Japanese and the American plant are not exactly alike. As if, by pronouncing the cabalistic word *species* the question were settled, or rather the greater part of it remanded out of the domain of science;— as if, while complete identity of forms implied community of origin, anything short of it carried no presumption of the kind; so leaving all these singular duplicates to be wondered at, indeed, but wholly beyond the reach of inquiry!

Now the only known cause of such likeness is inheritance; and as all transmission of likeness is with some difference in individuals, and as changed conditions have resulted, as is well known, in very considerable differences, it seems to me that, if the high antiquity of our actual vegetation could be rendered probable, not to say certain, and the former habitation of any of our species or of very near relatives of them in high northern regions could be ascertained, my whole case would be made out. The needful facts, of which I was ignorant when my essay was published, have now been for some years made known, — thanks, mainly, to the researches of Heer upon ample collections of arctic fossil plants. These are confirmed and extended by new investigations, by Heer and Lesquereux, the results of which have been indicated to me by the latter.

The Taxodium, which everywhere abounds in the miocene formations in Europe, has been specially identified, first by Gœppert, then by Heer, with our common cypress of the Southern States. It has been found, fossil, in Spitzbergen, Greenland and Alaska,

* Die Vegetation der Erde nach ihrer klimatischen Anordnung. 1871.

— in the latter country along with the remains of another form, distinguishable, but very like the common species; and this has been identified by Lesquereux in the miocene of the Rocky Mountains. So there is one species of tree which has come down essentially unchanged from the tertiary period, which for a long while inhabited both Europe and North America, and also, at some part of the period, the region which geographically connects the two (once doubtless much more closely than now), but has survived only in the Atlantic United States and Mexico.

The same Sequoia which abounds in the same miocene formations in Northern Europe has been abundantly found in those of Iceland, Spitzbergen, Greenland, Mackenzie River and Alaska. It is named *S. Langsdorfii*, but is pronounced to be very much like *S. sempervirens*, our living redwood of the Californian coast, and to be the ancient representative of it. Fossil specimens of a similar, if not the same, species have recently been detected in the Rocky Mountains by Hayden, and determined by our eminent palæontological botanist, Lesquereux; and he assures me that he has the common redwood itself from Oregon in a deposit of tertiary age. Another *Sequoia* (*S. Sternbergii*) discovered in miocene deposits in Greenland, is pronounced to be the representative of *S. gigantea*, the big tree of the Californian Sierra. If the Taxodium of the tertiary time in Europe and throughout the Arctic regions is the ancestor of our present bald cypress,— which is assumed in regarding them as specifically identical, — then I think we may, with our present light, fairly assume that the two redwoods of California are the direct or collateral descendants of the two ancient species which so closely resemble them.

The forests of the Arctic zone in tertiary times contained at least three other species of Sequoia, as determined by their remains, one of which, from Spitzbergen, also much resembles the common redwood of California. Another, "which appears to have been the commonest coniferous tree on Disco," was common in England and some other parts of Europe. So the Sequoias, now remarkable for their restricted station and numbers, as well as for their extraordinary size, are of an ancient stock; their ancestors and kindred formed a large part of the forests which flourished throughout the polar regions, now desolate and ice-clad, and which extended into low latitudes in Europe. On this continent one species, at least, had reached to the vicinity of its present habitat

before the glaciation of the region. Among the fossil specimens already found in California, but which our trustworthy palæontological botanist has not yet had time to examine, we may expect to find evidence of the early arrival of these two redwoods upon the ground which they now, after much vicissitude, scantily occupy.

Differences of climate, or circumstances of migration, or both, must have determined the survival of Sequoia upon the Pacific, and of Taxodium upon the Atlantic coast. And still the redwoods will not stand in the east, nor could our Taxodium find a congenial station in California.

As to the remaining near relative of Sequoia, the Chinese Glyptostrobus, a species of it, and its veritable representative, was contemporaneous with Sequoia and Taxodium, not only in temperate Europe, but throughout the Arctic regions from Greenland to Alaska. Very similar would seem to have been the fate of a more familiar gymnospermous tree, the Gingko or Salisburia. It is now indigenous to Japan only. Its ancestor, as we may fairly call it, since, according to Heer, "it corresponds so entirely with the living species that it can scarcely be separated from it," once inhabited Northern Europe, and the whole Arctic region round to Alaska, and had even a representative farther south, in our Rocky Mountain district. For some reason, this and Glyptostrobus survived only on the shores of Eastern Asia.

Libocedrus, on the other hand, appears to have cast in its lot with the Sequoias. Two species, according to Heer, were with them in Spitzbergen. Of the two now living, *L. decurrens*, the incense-cedar, is one of the noblest associates of the present redwoods; the other is far south in the Andes of Chili.

The genealogy of the Torreyas is more obscure; yet it is not unlikely that the yew-like trees, named Taxites, which flourished with the Sequoias in the tertiary Arctic forests, are the remote ancestors of the three species of Torreya, now severally in Florida, in California, and in Japan.

As to the pines and firs, these were more numerously associated with the ancient Sequoias of the polar forests than with their present representatives, but in different species, apparently more like those of Eastern than of Western North America. They must have encircled the polar zone then, as they encircle the present temperate zone now.

I must refrain from all enumeration of the angiospermous or ordinary deciduous trees and shrubs, which are now known, by their fossil remains, to have flourished throughout the polar regions when Greenland better deserved its name and enjoyed the present climate of New England and New Jersey. Then Greenland and the rest of the north abounded with oaks, representing the several groups of species which now inhabit both our eastern and western forest districts; several poplars, one very like our balsam poplar, or balm of Gilead tree; more beeches than there are now, a hornbeam, and a hop-hornbeam, some birches, a persimmon, and a planer-tree, near representatives of those of the Old World, at least of Asia, as well as of Atlantic North America, but all wanting in California; one Juglans like the walnut of the Old World, and another like our black walnut; two or three grapevines, one near our Southern fox grape or Muscadine, another near our Northern frost grape; a Tilia, very like our basswood of the Atlantic States only; a Liquidambar; a Magnolia, which recalls our *M. grandiflora;* a Liriodendron, sole representative of our tulip-tree; and a sassafras, very like the living tree.

Most of these, it will be noticed, have their nearest or their only living representatives in the Atlantic States, and when elsewhere, mainly in Eastern Asia. Several of them, or of species like them, have been detected in our tertiary deposits, west of the Mississippi, by Newberry and Lesquereux.

Herbaceous plants, as it happens, are rarely preserved in a fossil state, else they would probably supply additional testimony to the antiquity of our existing vegetation, its wide diffusion over the northern and now frigid zone, and its enforced migrations under changes of climate.

Concluding, then, as we must, that our existing vegetation is a continuation of that of the tertiary period, may we suppose that it absolutely originated then? Evidently not. The preceding cretaceous period has furnished to Carruthers in Europe a fossil fruit like that of the *Sequoia gigantea* of the famous groves, associated with pines of the same character as those that accompany the present tree; has furnished to Heer, from Greenland, two more Sequoias, one of them identical with a tertiary species, and one nearly allied to *Sequoia Langsdorfii*, which in turn is a probable ancestor of the common Californian redwood; has furnished to Lesquereux in North America the remains of another ancient

Sequoia, a Glyptostrobus, a Liquidambar which well represents our sweet-gum tree, oaks analogous to living ones, leaves of a plane-tree, which are also in the tertiary and are scarcely distinguishable from our own *Platanus occidentalis*, of a magnolia and tulip-tree, and "of a sassafras undistinguishable from our living species." I need not continue the enumeration. Suffice it to say that the facts justify the conclusion which Lesquereux — a very scrupulous investigator — has already announced: "that the essential types of our actual flora are marked in the cretaceous period, and have come to us after passing, without notable changes, through the tertiary formations of our continent."

According to these views, as regards plants at least, the adaptation to successive times and changed conditions has been maintained, not by absolute renewals, but by gradual modifications. I, for one, cannot doubt that the present existing species are the lineal successors of those that garnished the earth in the old time before them, and that they were as well adapted to their surroundings then, as those which flourish and bloom around us are to their conditions now. Order and exquisite adaptation did not wait for man's coming, nor were they ever stereotyped. Organic nature — by which I mean the system and totality of living things, and their adaptation to each other and to the world — with all its apparent and indeed real stability, should be likened, not to the ocean, which varies only by tidal oscillations from a fixed level to which it is always returning, but rather to a river, so vast that we can neither discern its shores nor reach its sources, whose onward flow is not less actual because too slow to be observed by the *ephemeræ* which hover over its surface, or are borne upon its bosom.

Such ideas as these, though still repugnant to some, and not long since to many, have so possessed the minds of the naturalists of the present day, that hardly a discourse can be pronounced or an investigation prosecuted without reference to them. I suppose that the views here taken are little, if at all, in advance of the average scientific mind of the day. I cannot regard them as less noble than those which they are succeeding.

An able philosophical writer, Miss Frances Power Cobbe, has recently and truthfully said:*

* Darwinism in Morals; in Theological Review, April, 1871.

"It is a singular fact, that when we can find out how anything is done, our first conclusion seems to be that God did not do it. No matter how wonderful, how beautiful, how intimately complex and delicate has been the machinery which has worked, perhaps for centuries, perhaps for millions of ages, to bring about some beneficent result, if we can but catch a glimpse of the wheels its divine character disappears."

I agree with the writer that this first conclusion is premature and unworthy; I will add deplorable. Through what faults or infirmities of dogmatism on the one hand and skepticism on the other it came to be so thought, we need not here consider. Let us hope, and I confidently expect, that it is not to last;—that the religious faith which survived without a shock the notion of the fixity of the earth itself, may equally outlast the notion of the absolute fixity of the species which inhabit it;—that, in the future even more than in the past, faith in an *order*, which is the basis of science, will not (as it cannot reasonably) be dissevered from faith in an *Ordainer*, which is the basis of religion.

THE WHITE COFFEE-LEAF MINER.

[Concluded from June number, p. 341.]

BY B. PICKMAN MANN.

Abundance.— Some idea of the abundance of these insects may be given by stating that, although, as I was frequently told, they were much less destructive than usual during the year in which I observed them, yet from one tree, which I chose for an experiment as not exceptional unless by reason of its size, I picked one hundred and fifty-three leaves in the course of nineteen minutes, endeavoring at the same time to select only those leaves which contained living larvæ, and to leave those from which the larvæ had escaped. Of these leaves forty-four contained recent mines, but the larvæ had escaped; ninety contained one hundred and twenty-two mines still inhabited; the rest contained old mines or blotches made by a fungus which also attacks the leaves.

Manner of Devastation.—The injury caused by this insect is due to the destruction of the digestive and respiratory organs of

the plant. The larva "absorbs the sap, obstructs the circulatory channels, and impedes the vegetable respiration" (Madinier, l. c. p. 33), thus depriving the plant of its food, or preventing the food from becoming fit to sustain life, in consequence of which the plant becomes exhausted, and either dies, or bears fewer and smaller fruit.

Amount of Devastation. — Guérin says (Mém. etc. p. 12; [Dumeril, Rapp.,] p. 33) that in the Antilles "all the coffee-trees were feeble and languishing: they bore only small and stunted fruits, their leaves were spotted or blackened, in [great] part dried up, and although dead, remaining upon the branches, * * which rendered these shrubs languishing, and had even caused the death of many of them." Madinier says (l. c. p. 33) that owing to the attacks of insects, of which this is the most noxious, the culture of the coffee-tree was abandoned in the island of Martinique. This insect is said to lessen the coffee-crop of Brazil by at least one-fifth.

Enemies: Fungus.— The leaves of the coffee-tree sometimes turn yellow at the tip or some portion of the edge. The spots thus formed increase in size until they cover the whole leaf, gradually turning to a brown color, by which time the leaf has become dried up. These spots may be easily distinguished from those made by the larva, because the two skins of the leaf which is attacked by this disease cannot be separated, and the color is more uniform, appearing equally on both surfaces of the leaf. I was told that this was the work of a fungus. It attacks leaves which have or have not been injured by the larva, but seems to find more ready lodgment on such part of the leaf as has been injured previously. It appears in these cases to kill the larva within the mine, as many mines recently begun are found to contain the flat and empty skin of the larva, with no indication of another destroyer, but I may have been misled in my judgment by seeing the interrupted labor of the Eulophus of which I will speak next.

Enemies: Parasites. — I have found two ichneumons parasitic upon the insect: one upon the larva, the other upon the pupa. I have also found great numbers of mites (Acarina) living in the mines. The first of these ichneumons, which feeds upon the larva of *Cemiostoma coffeellum*, was found several times under a small roundish blotch of a grayish-brown color (about the same in color as the fungus-spot), which was dotted with black dots, as

if a lichen had grown upon it. These black dots may have been the frass of the larva, which showed through the epidermis.

Where the ichneumon had escaped from these dotted blotches, it had cut out an oval or rounded hole in the upper surface of the blotch. Once, before I broke open one of these mines from which the ichneumon had escaped, I found the pupa-skin *in situ*, with its broken end just touching the hole, through which it could be seen. The hole was about 0·2 millimeter in diameter.

I found several of these blotches which had no hole in them, and yet I found no ichneumon within, but the larva-skin flat and empty. This led me to think that the fungus which I have mentioned may kill and exhaust some larvæ.

According to Ratzeburg (Ichneumonen der Forstinsecten, i, (1844), p. 158), this ichneumon belongs to the genus Eulophus, in the family of *Chalcididæ*. I have not been able to make a satisfactory figure of it, owing to the injured condition of the only three examples which I succeeded in preserving. It may be called *Eulophus cemiostomatis*, if it has not been previously described.

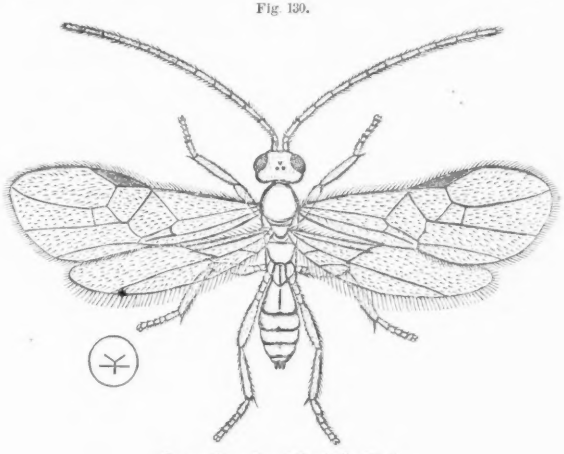
The imago is metallic green or coppery; the wings are transparent, somewhat iridescent; the fore wings crossed by a brownish cloud beyond the middle. The fore wings have no other vein than a double one near the front margin, which is bent at about one-quarter, and ends in a fork at about three-quarters of the distance between the base and the tip, sending one prong of the fork in line with the vein, and the other towards the inner angle of the wing. The inner margin of the fore wing is also thickened for a short distance near the middle; and the front margin of the hind wing is thickened along more than half its length from the base. All the wings are fringed around their margins, and the wings as well as the different parts of the body and legs are pubescent. The antennæ are eight-jointed, thinly haired; the first joint long, enlarged at the apex; the last three joints forming an ovate-conical club; the intermediate four joints ovate-cylindrical. The abdomen is elongate-oval, attached to the thorax by a broad neck, and is turned up at the sides. The tarsi are four-jointed. The length of the head and body is about 0·8 millimeter, the expanse of wings about 1·5 millimeters.

The pupa when seen through the pupa-skin seems to be longer than the imagos.—The pupa-skins look large; all are alike; one or two were sufficiently transparent to allow the occu-

pant to be seen, which was much narrower than the pupa-skin. From one pupa-skin I hatched one of these ichneumons. I found them during all the time (April to June) in which I studied the Cemiostoma. In the one hundred and fifty-three leaves mentioned I found eight mines containing these insects. Afterwards I obtained two from a box containing leaves.

The second ichneumon parasitic on the *Cemiostoma coffeellum*, inhabits the larva while it is still in the mine, as I learned by finding an immature example dead within the thin and dried skin of a nearly full-grown larva, but I believe that it usually does

Fig. 130.



Bracon Parasite of the Coffee Moth.

not kill its victim until after the Leaf-miner has become a pupa. It then completes its work of destruction and cuts a hole in the upper side of the cocoon, through which it escapes.

It belongs to that subfamily of the ichneumons called *Braconidæ;* consequently I will call it *Bracon letifer;* but as far as I have examined its characters, it corresponds more nearly to the genus Rogas than to any other genus described in Curtis' "British Entomology," and seems to be congeneric with, and judging by the venation of the wings, nearly related to *Exothecus exsertor*, as given in Wesmael's "Monographie des Braconides de Belgique" in the Nouvelles Mémoires de l'Academie de Bruxelles, xi (1838), p. 73, and accompanying plate, fig. 10.

The perfect insect (Fig. 130) is honey yellow, except the eyes, ocelli, and sometimes the ocellar space, which are black. The antennæ are also black, and consist of twelve slender, sub-equal, uniformly cylindrical joints beyond the scape, which seems to consist of two short, stout joints, making fourteen in all. The head is transverse; the front projects slightly beyond the eyes; the hind margin of the vertex is emarginate, the emargination filled with the upper edge of the occiput. The three ocelli are arranged triangularly; in some examples they are approximate, in others distant, which may be a sexual difference. The neck is distinct; the back of the mesothorax sub-hexagonal, rounded, tapering anteriorly. In some of my examples the abdomen is elongate, subclavate; in others it is rotundate. I think the shape given in the figure is the most lifelike. In some examples the veins near the extremity of the wings are very feebly developed. The wings are fringed. The legs, abdomen, thorax, head, antennæ and wings are pubescent. Length 1·9 millimeters; expanse 4·7 millimeters. The immature example, or pupa of the *Bracon*, which I found as stated above, had wing pads instead of wings. These were dark, smoky black, 0·5 millimeter long. The antennæ are honey yellow, instead of black, as in the developed specimens. They lie along the breast, and reach to the end of the posterior femora, which is about the length of the whole body. The first two joints are retracted within a cavity in the front of the head, which seems to be the result of a doubling in of the front. Only the two posterior ocelli are visible, distant, and though enough of the front remains to contain the anterior ocellus, it seems as if it must be still farther forward than in the imago. The hind pair of legs is stuck straight out behind. (The abdomen is broken off.) The first and second pair of legs have the femur folded forwards; the tibia and tarsus lie towards the end of the body. This example lay within the skin of the larva, with its head towards the head of the larva.

A possible enemy is a greenish-yellow spider which draws down the edge of a coffee-leaf on the under side, and spins a light web from this edge to the surface of the leaf, leaving each end of the nest open.

Geographical Distribution. — As we have stated, M. Perrottet met these insects in the Antilles; M. Madinier found them in the island of Martinique; and I observed them in the Province of

Rio de Janeiro. They are said to extend over the whole coffee-region of Brazil.

History. — Dr. Christovão, and his brother Col. Antonio Corrêa e Castro told me that the coffee-trees were first introduced into Brazil by the Brazilian Minister at Paris, who sent two plants to the city of Rio de Janeiro, where they were planted on Mount Tijuca. From these two plants many others were obtained, which were kept in gardens as ornamental shrubs. Some of these were afterwards distributed to the plantations to be cultivated for commercial purposes. From them sprung the coffee-plantations of Brazil. Until about twenty years ago these plantations were free from all noticed pests. About that time, owing to the general exhaustion of the coffee-trees through long bearing, the Government imported quantities of new plants from the Antilles and from the isle of Bourbon, and distributed them all over the country. It was noticed during the very next year that the leaves of the coffee-tree were attacked by the larva of the moth, whose history is given here, which has ravaged the coffee-plantations of Brazil ever since. It cannot be doubted that the insects were brought from the Antilles with the plants, and that a proper examination of the plants at that time, by any person familiar with the appearance and habits of the enemies of the coffee-tree, would have prevented the introduction of so great a pest.*

Remedies. — The entomologist, like the physician, finds it much

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* *Bibliography.* 1. Guérin-Méneville et Perrottet. Mémoire sur un Insecte et un Champignon qui ravagent les Caféiers aux Antilles. Paris. Ministère de la Marine. 1842. 8vo. pg. 40. tab. 2. Gives the history of *Elachista coffeella*.

*2. Revue Zoologique. 1842. p. 126-127. Contains a notice of No. 1.

*3. Annales de la Société Entomologique de France. 1842. T. XI, Bulletin, p. II. Contains a notice of No. 1.

*4. Zeller. Linnæa Entomologica. 1848. T. III, p. 250, 272-273; T. II, tab. II, fig. 37-39. Establishes the genus *Cemiostoma*.

*5. Stainton. The Natural History of the Tineina. 1855. Vol. I, p. 284-334. tab. 1. Contains "General Observations on the genus *Cemiostoma*," and the history of *C. spartifoliellum*, *laburnellum*, and *scitellum*.

*6. Nietner. Observations on the Enemies of the Coffee-tree in Ceylon. Ceylon. Published at the Ceylon Times Office. 1861. 8vo. pg. 31. On p. 24, mentions *Elachista coffeella*.

*7. Stainton. The Entomologist's Weekly Intelligencer for 1861. Vol. X, p. 110-111. "A few words respecting *Cemiostoma coffeella*; an insect injurious to the Coffee plantations of the West Indies."

8. Madinier. Revista Agricola do Imperial Instituto Fluminense de Agricultura. No. 3. p. 29 et seq. Brief notice of the Coffee-tree, containing on p. 33 an account of the habits of an insect called "*noctuella*," which must be the *C. coffeellum*.

(The asterisks before the titles of the above works indicate that I have taken the titles and references directly from the works cited).

more difficult to choose a proper remedy for a disease with which he is familiar, than to trace out the nature and progress of the disease. But at this day the science of entomology, particularly in its practical application, is of such recent origin, that it suffers under a disadvantage from which the practice of medicine is free, namely, it possesses no treasury of results drawn from experience. The practical entomologist can only recommend measures to be put to the trial, and in this way gradually gather a body of experimental facts which may serve as a guide in the future.

The most obvious remedy which suggests itself is the collection and destruction of the leaves which contain the living larvæ. If this was done thoroughly, it would no doubt result in the complete extermination of the pests, a result the value of which would be incredible.

Towards this end Guérin recommends (Mém., etc., pp. 18-20) that "the branches of the coffee-trees which are loaded with [infested] leaves should be cut off in all parts of the country at one time, and burned, while the insect is in the larva state." If this were done, he says, "these coffee-trees should be cut down in such manner that the vegetation could resume its ordinary course shortly after the operation, to the end, if it were possible, of not having to regret but one crop of coffee. * * To attain this condition more promptly, the operation in question should be made with a cutting instrument, and at a height which should be determined by the proprietor himself (a metre and a half). Care should be taken to preserve here and there some young and vigorous branches, which would tend to maintain the equilibrium of the sap in all parts of the plant. * * Afterwards the development of new leaves ought to be watched with the greatest exactness, and if there should appear from place to place some spotted leaves, they should be destroyed promptly."

It will be seen that Guérin expects to cause the loss of one crop of coffee in his endeavor to exterminate the insect. Certainly the issue of the experiment, if successful, would be well worth the loss of an entire crop, but I think the same result could be obtained in a preferable way: entailing much more labor, but avoiding at the same time the loss. Probably not a single branch would be free from infested leaves, so that it would be necessary to find some other means of killing the larvæ in the leaves of those branches which remained. On the other hand, many healthy

leaves would be lost, if whole branches were cut off. I think it would be better to pick off all the infested leaves, and burn them, leaving the healthy leaves to support the tree. Guérin says [Mém. etc., p. 19] that "the epoch which it would seem ought to be the most favorable for this operation would be that which immediately follows the winter season, or that during which the temperature is the lowest, because the larva finds itself then as it were benumbed, and cannot be transformed into a moth until the return of a softer temperature." The time appointed for picking off the leaves might be, for the obvious purpose of saving labor, that at which the smallest number of old leaves remain upon the trees, if there is any such time. If the leaves were picked at such time as to take the greatest number of larvæ when they were about two weeks old, it would not be difficult to select them, as the size of the blotches would make them very noticeable.

I have made a theoretical estimate of the expense which would be incurred in picking off the leaves as I recommend, and of the relative increased yield of coffee which would result on a plantation of given size. Testing this theory by the numerical data given to me by Dr. Christovão Corrêa e Castro, and making large allowances for unfavorable circumstances, I find that the expense would be more than met by the next year's crop; but even if this should not be the case in the second year, it must be remembered that such a thorough and expensive war upon the insects never need be made more than once, and that with vigilance the trees could be kept in good order and the *increased yield maintained* continually afterwards. But vigilance must be exercised. One picking would not entirely exterminate the insects, however thoroughly it were done. The planters should also make experiments at all times, and seek other means of destroying not only this but all the enemies of their crops. They alone have the facilities for increasing and utilizing all the knowledge which can be gained upon these subjects.

Another remedy which Guérin recommends is "to kindle fires at all points of the coffee-plantations, at the time when the moths begin to issue from their cocoons. It is well known that many insects, and above all the nocturnal Lepidoptera, are attracted by light, and come whirling around a fire until they are burned there. Certainly a great number of individuals would be thus destroyed. At the same time, and to attain this object more promptly, lighted

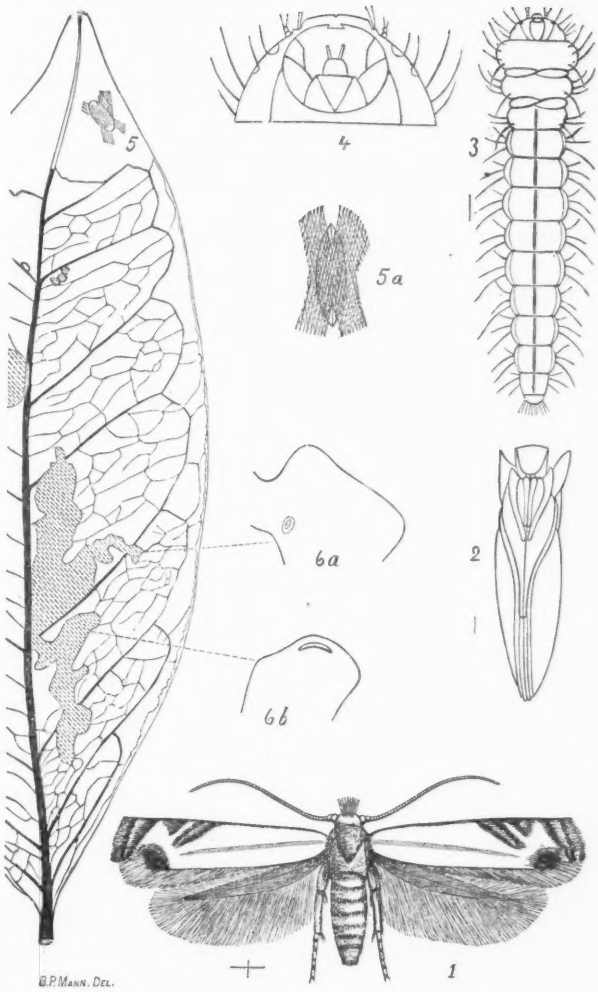
torches might be carried through the plantations in the evening. Thus a crowd of moths, hidden in places to which the light of fixed fires could not penetrate, would be attracted, and killed." Although this measure would not be absolutely efficacious, unless practised for a long time and together over the whole country, which would be difficult to accomplish, yet it would repay all the labor expended upon it, if adopted at the time when the moths were abundant.

I will also recommend again the use of Col. Sorsby's process, which I have described at length in my report upon the enemies of Maize, drawing the description of it from the Report of the United States Commissioner of Patents for the year 1854, Part iii, p. 65.

I have thus stated all the direct means which have been suggested for the suppression of these pests. It is evident that none of them can be adopted without the expenditure of much time and labor. If other measures, less direct, could be employed, which would add to the efficiency, or take the place of those already suggested, the benefit would be great. Whatever measures are employed, however, must be founded upon certain general principles, in order to insure their success. What those principles are must first be learned, and then in what detail they can be applied. It is only in this way that we can determine whether there is any mode of opposing our insect enemies which will not cost more than it is worth.

Men gain time to advance in civilization and prosperity, by mastering the laws of nature, and converting natural forces into tools which do their work automatically as it were. Nature has provided enemies for the moth whose history we have been studying. Let us only learn how to cherish and encourage these natural friends of ours, and they will work for us thoroughly. They were made to work for us whenever we should learn how to command them.

I recommend that before the picked leaves are burned they be placed in an apartment from which the moths cannot escape, and there allowed to lie until the insects have developed. If such an apartment should be made with sides of glass, and a properly guarded entrance, it would be easy to capture the parasites while they rested on the glass, and to liberate them in the plantations, or transport them to other parts of the country where they might be needed more. At the same time the moths could be caught



MANN. ON THE WHITE COFFEE-LEAF MINER.

(Corrected.)

and killed. Or the sides of such an apartment could be made of gauze, fine enough not to allow the passage of the moths, but yet large enough to let the parasites out. Such a building could be placed in the midst of a plantation. I believe that eventually we shall have to rely mainly upon such indirect measures as a protection for our crops. It might even be worth while to undertake a positive cultivation of the parasites, at least at those times when the race has greatly diminished in numbers. It has often been observed, in studying the history of those insects which are subjected to unnatural conditions by man's cultivation of the ground, that there is an alternation of years or of series of years in which the insects are found to be very destructive, or to have almost entirely disappeared. These alternations are partly due to the influence of the seasons, but largely to the attacks of other insects. At first the destructive insects are found to be very numerous, but an examination will show that they have already been attacked by parasites which kill them, while the parasites themselves develop. This process goes on until the parasites have so far outnumbered their prey as nearly to exterminate them, when they will no longer be able to find food, and will themselves perish. Then once more the destructive insects will have an opportunity to multiply, and so the rotation will be continued. Now it is at the time when the destructive insects have been reduced to the smallest numbers that the enlightened agriculturist will find it most practicable to adopt such measures that their numbers may never again increase. Knowing how rapidly these insects increase, when not held in control by the forces of nature, he will feel that every effort of his to stop them at the first step will be an investment of labor at compound interest for a long time to come. Who then would count the trouble? But he must know what to do.

P. S.—I desire to correct an error in the former part of this biography, kindly pointed out to me by Mr. V. T. Chambers, of Covington, Kentucky, in the current volume of the AMERICAN NATURALIST, p. 489–490. On p. 338, I said that *C. coffeellum* was the only species of the genus known outside of the limits of Europe. This is a mistake. While I was in Brazil, Mr. Chambers described in the Canadian Entomologist, iii (1871) p. 23–25, a species from the United States, called *C. albella*.

As all but one of Mr. Chambers' references, in his note of correction, were wrong, I must, in order to be able to compare his species with the others of the genus, suppose it also due to negligence that he (through Mr. Stainton) describes the silvery gray metallic spot of the fore wings as apical, instead of at the inner angle. If this supposition is correct, *C. albellum* seems more nearly related to *C. coffeellum* than any of the other species, but may be known from it by having the spot at the inner angle of the fore wings silvery gray metallic, with very distinct black margins before and

behind, and an indistinct pale golden streak along the base of the fringe from the costa not quite to the inner angle; while it seems not to have the two oblique lines of black scales described in *C. coffeellum*, nor the golden band which partially surrounds the spot in that species.

Mr. Chambers says also, in his note of correction, that "in the Transactions of the London Entomological Society, Ser. 2, Vol. v, pp. 21 and 27, and in Ser. 3, Vol. ii. p. 101, certainly two, and if my [his] memory is not at fault, three species [of *Cemiostoma*], are described from India." I have examined the pages to which he evidently intends to refer, and find that both the species mentioned, *C. wailesellum* and *C. lotellum*, are said to come from England.

I have had a new edition of the accompanying plate struck off, because the former one contained some errors introduced by the artist, who transferred my figures from paper to wood. Some of the figures are incomplete, because I have only drawn what I could see. This is especially the case with the larva.

ON THE OCCURRENCE OF FACE URNS IN BRAZIL.

BY PROF. CHARLES FRED. HARTT.

On my visit last year to Brazil, my good friend, Senhor Ferreira Penna, showed me in the Museum of Pará a remarkably fine, well-preserved, and curiously-shaped burial vase of the class called by the Germans *gesichtsurnen*, or *face urns*, which had been obtained from a cave on the Rio Maracá, a little river in the Province of Pará, emptying into the Amazonas some fifty miles above Macapá. Of this urn, at his desire, I made the accompanying rough sketch with a few notes for publication in the NATURALIST.

The urn was intended to represent a human figure sitting on a low bench or stool. The body is cylindrical and, including the stool, is just about two feet in height.* Its diameter is about 9 inches. The legs spring from the body at a distance from its base equal to about one-fifth the height of the body. They are very short, small, cylindrical and hollow. They bend slightly to represent the knee, below which is a broad constriction intended for an ornament. Below this the leg swells to a ball as represented in the engraving. The feet are flat, irregular in shape, cut off squarely in front and furnished with six toes each. They are so constructed as to rest on the ground. The arms have their origin at a distance from the top of the body of the urn less than a

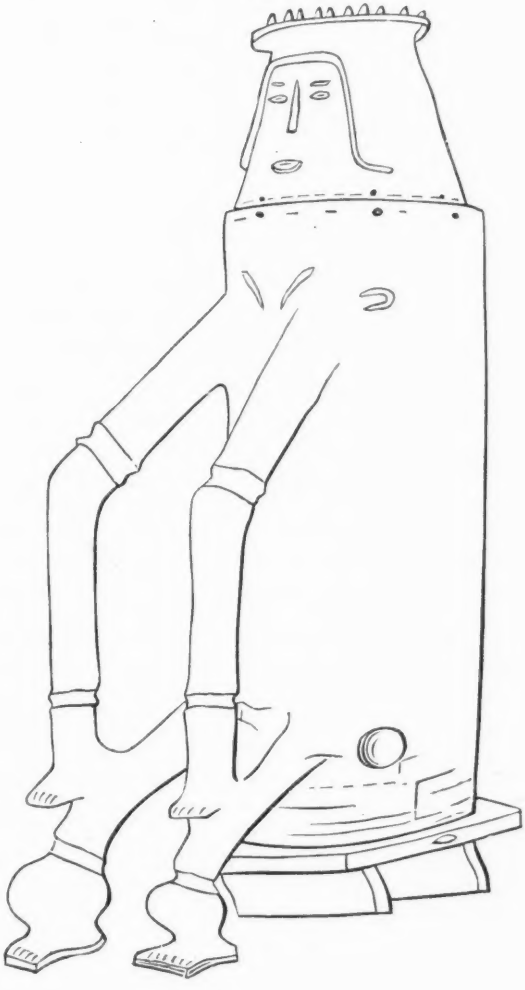
*The measurements given in this article are approximate, but were carefully estimated.

quarter of its height. They extend downward at an angle of 45°, more or less, and diverge a little. They bend abruptly downward at the elbow, the fore arm being perpendicular. The hands, resting on the knees, project forward like feet and are cut off squarely, the fingers, five in number, being indicated by scratches. This awkward turning forward of the elbows recalls the similar position of the arm of an Indian warrior, in the well known picture of the "Marriage of Pocahontas." The arms of the vase are cylindrical like the legs and not only longer but thicker than they. An armlet is represented just above the knee-like elbow, and a bracelet two or three inches above each wrist. Just above and between the shoulders are two short prominent ridges, shown in the engraving, which may be intended to represent clavicles. On each side, just back of the shoulder, is a similar ridge curved into a loop, the two ends of which are turned forward. On the back, coinciding nearly with the middle third of the mesial line is a thin finlike crest ornamented with lines drawn perpendicularly to the body. The figure is furnished with a carefully moulded, erect phallus. On the thigh is a low, cylindrical prominence, concave on the summit, shaped like the centrum of an ichthyosaur.

The head, answering as a cover, is in a separate piece, forming a hollow truncated cone 9–10 inches in height, the base being in outside measurement a little smaller than that of the upper part of the body of the urn. The top of the head is flat, with a projecting rim like a narrow brimmed hat and on the surface are a large number of sharp points, arranged in regular quincunx order. On the front of the cover is represented a face, the general arrangement of whose features is well shown in the sketch. The bounding line, eyes, eyebrows, nose and mouth are all in high relief and were applied after the head-like cover had been moulded. Around the base of the head-like cover are six holes to which other perforations in the rim of the base correspond. These were intended for strings used in tying on the cover, after which a brown wax was used to lute the two together. Of this material a portion still remains and bears the impress of what appears to be palm straw.

The base on which the figure sits is square, solid and supported by two upright, transverse pieces like sled runners; the whole resembling one of the curious stools hewn out of a solid block and used by the Amazonian Indian nowadays.

Fig. 131.



The material of which the urn is composed is a very coarse clay full of sand and consequently brittle when burned. The legs and arms are broken in several places. The surface of the vase is moderately smooth and the greater part is without ornamentation. The face has received a wash of ochre yellow clay. Near the base are a few white lines difficult to trace out, but which resemble the ornament on the burial vase, Fig. 65, in my little paper "On the Pottery of Marajó," in the NATURALIST for July of last year.

The urn, as it at present exists in the Museum of Pará, contains part of a human skeleton, showing no trace of burning. The cranium is wanting. It is impossible now to say whether the vase ever contained the whole skeleton. From the small size of the urn I should suppose, however, that it did not.

Senhor Penna visited, in February of this year, the locality where this curious urn was found and sent me a few notes on it. He says that several leagues up the Rio Maracá, at a short distance from the river, there is an immense flat mass of friable sandstone, in which is a large crevice, expanding, in one place, into a large grotto. Near by and on a plain, to reach which it is necessary to climb a steep hill covered by wood, is another small grotto lighted by a large opening above. On the floor of this last grotto, Senhor Penna found several urns, mostly broken, but of which two were entire. Some of these were of the same form as that I have just described, but others were shaped like armadillos and tortoises (*Jabuti*), though all had human countenances. Senhor Penna says that all the tubular vases, like the one just described, have the organs of sex, male or female, carefully and prominently represented.

Since the above was sent to press I have succeeded in restoring a magnificent female *gesichtsurnen* from the Ilha do Pacoval in Lake Arary, Marajó. In this urn the upper part is rounded so as to represent a head with human features. Besides this there occur in the collection made last winter, by my assistant, Mr. Derby, fragments of two other urns of the same class, one of which is furnished with *two* faces on opposite sides of the urn. These urns together with the new collections will shortly be described.

ON THE GEOLOGY OF THE ISLAND OF AQUIDNECK AND THE NEIGHBORING PARTS OF THE SHORES OF NARRAGANSET BAY.*

[Concluded from page 528.]

BY PROF. N. S. SHALER.

GLACIAL DEPOSITS AND ICE MARKS.—The contour of a surface alone is generally sufficient to establish the former existence of glaciers, if they have ever worked upon it, but it is not to these indications alone that we must look for the evidence of the work of this great agent in this region. Every mass of rock exposed to view shows the rounded, smoothed and scored surface so characteristic of ice work. Every part of the island, level enough to carry such material, is buried beneath a coating of detrital material from two to forty feet in thickness. We propose to study these deposits of glaciated matter with a view to determine some of the more important features connected with the work done by moving ice.

These deposits have a composition which varies considerably according to the position in which they are found. At the extreme northern end of the island, all the pebbles found in them are from rocks which belong beyond its limits. The greater part of the pebbles can be referred to rocks which are in place on the shores of Mount Hope Bay to the northward, though some seem to come from points as far up as the neighborhood of Taunton. Only a small part cannot be readily referred to materials in place in the basins of the streams which flow into Narraganset Bay. It is of course impossible to assert that none of these unreferred specimens came from more remote regions to the northward, but inasmuch as there is a very wide difference noticeable between the glacial material in the basin of the Charles River and the other streams which flow into Massachusetts Bay, and this drift on the north of Aquidneck island, there can be little doubt that the transportation of erratics, from limits more than fifty miles away, has been very slight indeed, if it occurs at all. A large part of the drift mass is made up of boulders of a conglomerate, which

*This paper is taken from a report made to Prof. Benj. Pierce, Superintendent of the United States Coast Survey, and is published by his permission.

reminds one of the pudding stone in the neighborhood of Boston, but it differs from that rock in the comparative abundance of the matrix, and scarcity of pebbles of large size. The pebbles are, moreover, composed of different sorts of rock from those in the Boston conglomerates. After a careful inspection, I am compelled to conclude that none of the conglomerate masses are from that set of beds, which extend over the country to the north and east of the Blue Hills near Boston.

On the northern part of the island, a few of the syenite cliffs, which rise at steep angles from the plain, are the only points which have no drift matter upon them. Between these cliffs and the north slope of Butts Hill is a plain much indented by singular depressions already described (*vide supra* p. 523). The drift here has an unknown depth; it is known to be more than fifteen feet in thickness, and is probably at some points as much as forty feet thick. The pebbles here are with very rare exceptions less than six inches in diameter and are heaped together with only imperfect stratification. The cementing matter differs in no way from that which is always found in our boulder deposits, being a mixture of all the kinds of materials which go to make up the pebbly part of the deposit. There is no trace of true moraine heaps, such as fill the valleys in front of existing glaciers, in this part of the island. All the drift has, more or less, the character described by Agassiz, and by him referred to the melting of a great ice sheet, and the deposition, in a great unstratified mass, of all the pebbles which it had torn from its bed. Following the drift southwards, we find that without much change of volume the constituents become greatly altered in character. A large part of the materials which are found in it at the extreme northern end disappear, and are replaced by fragments from the rocks which belong in the immediate neighborhood. This is very clearly seen in the beds which overlie the portion of the island which contains coal. The matrix or cement of the mass is here much darker than in the drift near Blue Bill Cove; a large part of the fragments are of slate of a carbonaceous character. This blackish color extends to the drift found to the south of the coal field, though it becomes less and less marked as we go away from the source of supply of the coal. On the eastern shore we gradually leave the syenite boulders behind us as we go from north to south. Fragments of this sort of rock, which made up about one-half of the drift near

Anthony's, become rare when we get as far south as the glen, and are almost entirely wanting at Sachuest Neck. The conglomerate, with very much compressed pebbles, which does not occur north of the east and west line drawn from Black Point to Lawton's valley, is not found in the drift to the north of that line, though not infrequent in the beds of that age at all points to the southward of it. The north end of the island has a great quantity of conglomerate erratics but they belong to the type of that rock found to the north of Somerset on the Taunton River, and probably at other points in the direction whence came the glacial stream.

It will be shown further on that at various points the underlying rock of the island is traversed by veins of white quartz often having a thickness of many feet; the position of these veins, even when the rock in place is hidden, is often made evident by the trains of quartz blocks which lie mingled with the boulder mass, to the south of the outcrop of the vein. The actual thickness of the drift diminishes pretty steadily as we go southward; this is doubtless to be attributed to the soft character of the rock which underlies the island. That which the glacial stream rent from the bottom, in that part of its course which lies just north of the island, was of a much more resisting character than that which it encountered in its course over the island itself; naturally the endurance of the rock masses ground beneath the glacier was in proportion to the hardness of the material of which they were composed, so that where the rock was very soft the rapid disappearance of its fragments would prevent a great accumulation of débris.

Professor Agassiz has already called attention to the essential difference in the character of the débris of the wearing in the two modes in which glaciers may act. 1st, where the ice is in the form isolated streams bounded by distinct rock walls, as in the existing glaciers of Switzerland. 2d, where the ice is spread broadly over the surface of the continent, attaining to a depth which quite deprives it of all bounding walls, giving us a sea of glacial matter in place of the rivers of ice in the other condition. In the first condition of the ice, the rock boundaries of the streams furnish comparatively the larger part of the material transported by the glacier; something is torn away from the bed by the moving stream, but its comparatively thin mass gives it no such abrading

power as is possessed by the far thicker continental glacier. This last named form of the action can only take up masses of material from the base over which it grinds; its ability to rend fragments from the rock beneath it would always be far greater than the stream glacier, on account of its superior thickness and therefore far greater crushing power.

Examination of the materials deposited by these two forms of ice action shows us enough difference between them to enable us to determine to which of the two any given mass of glacial material belongs. Where a large part of the fragments are angular, showing no trace of grinding against the bottom of the glacier, we may assume that the ice which carried the material was a local stream which received the mass of its load from that part of the sides of the valley in which it flowed which were above the level of the ice; where on the contrary the whole of the débris is more or less rounded, a large part polished or scored, and all showing the effect of the abrasion which must occur when the fragments are dragged from their bed by the moving ice, we must conclude that the sheet had no side barriers from which a supply of débris could be furnished by falls and avalanches, as the existing glaciers in Switzerland are fed, but obtained their whole load from the bed rock.

I am well convinced that all the débris on the island of Aquidneck has been deposited by ice acting in the last of these two methods. Possibly there may have been a little action of a more local character, but the evidence forces us to the conclusion that the principal part of the drift found here has been deposited by the melting of a mass of ice in which it was held, rather than by ice streams heaping it up in the form of moraines. The probability that this was the method by which the glacial detritus was transported renders it the more likely that all the material is of local origin. Rock masses resting on the surface of a glacier are in a position favorable for transportation to the greatest distance that the stream can flow; not so the detritus which rests between the mass of ice and the bed rock. All the material torn up from its bed by the glaciers must remain permanently near to the same relative level in the ice mass, close to its base, for there is no possible means whereby they could become lifted into the upper parts of ice. Here they would be exposed to the wearing action which results from continual grinding against the bed of the stream or constant friction against each other; this must lead

to their speedy destruction. We thus see that by far the larger part of the eroded materials of the ancient continental glaciers must have been rapidly converted into mud which would have been easily swept away by the sub-glacial streams, which would have coursed beneath the great ice sheets of this region, just as they now flow beneath the Greenland glaciers and bear their tide of muddy water to the sea.

There are only two points where the glacial deposits seem capable of any other interpretation. Two-thirds of a mile north of the coal mines on the west side of the railway, there is a mass of drift having a form which strongly suggests that it might have been deposited as a terminal moraine. It is in the form of a ridge a few hundred feet long, and by its position, shape and structure is likely to have been the mass accumulated during the retreat of the ice stream when it paused for awhile with its termination at that point. The other point is at Portsmouth Grove, a few hundred feet to the south and east of the railroad station, where the drift is piled in a number of conical hills which lie in a general north and south direction. The internal structure is not shown by sections as at the other point, but there can be no reasonable doubt that the whole mass is of drift material. It is pretty clear that these hills have been formed in a different way from the rest of the drift; there seems no other reasonable explanation than to suppose that they were also formed by a retreating ice stream.

The evidence concerning the history of the glacial period, given by the remains it has left behind, is essentially the same as that which we find over the region to the northward. A period during which the ice sheet wrapped the land and extended far out to sea was succeeded by a time of rapid shrinking of the ice, rapid in a geological sense, though its complete passing away may have required several thousand years. During this period of retreat, the various channels of Narraganset Bay may have had more or less well defined glaciers, and these, though they would have gathered little débris upon their surface owing to the low nature of their bounding walls, would still heap up mounds at their terminations, at those points where their retreat may have been from time to time interrupted.

The actual amount of erosion during the last glacial period is not readily determined; there is some interesting evidence, however, to be gained from a study of certain instances of wear found

on Aquidneck Island. Wherever there is a mass of rock which juts abruptly from the general surface, it shows some of the peculiar marks of southward moving ice; besides the universal smoothing and scoring of the surface, each of these projections shows us the phenomena of "shock and lee sides," and the northern end is always more worn than the southern end. All the precipitous slopes of any considerable area are upon the faces away from the northern side of the masses to which they belong. This sort of evidence is visible in most regions which have been subjected to glacial action, but at two points on this island it takes a shape which has not been observed at other points on our coast. In the felsite district south of Newport Harbor there are many admirable specimens of rounded bosses of rock, the "roches moutonée" of the French geologists. These, one and all, show on careful study more or less of the shock and lee sides. There is one of these felsite masses which has its northern end riven into massive fragments which have been pushed around towards the south in such a fashion as to make the direction of the force very evident. We see here the prodigious rending force of the glacier, for there has been separated at one moment a mass of rock sufficient to furnish at least one hundred tons of boulders to the ice current.

The mass of conglomerate and associated materials known as Paradise Rocks also shows some interesting phenomena. These rocks consist of a set of ridges of steeply inclined beds of varying hardness, which owe their position to a number of parallel faults extending in a north and south direction with a considerable *throw*, so that the projecting edges of the rocks rise at sharp angles to the height of from fifty to one hundred and fifty feet above the sea level. Carefully tracing these rocks in the direction in which they are continued to the northward, it becomes evident that, at the time when they were formed, the ridges continued for several hundred feet to the northward of the base of the slopes which lead down to the comparatively low land which now bounds them on that side. We cannot resist the conviction that the powerful agent which has cut away these solid masses of rock was the ice stream which has so clearly scored their surfaces and left the marks of its power on every square foot of their remaining surfaces.

Very little of the surface of the island remains in precisely the condition in which it was at the time of the coming of the white

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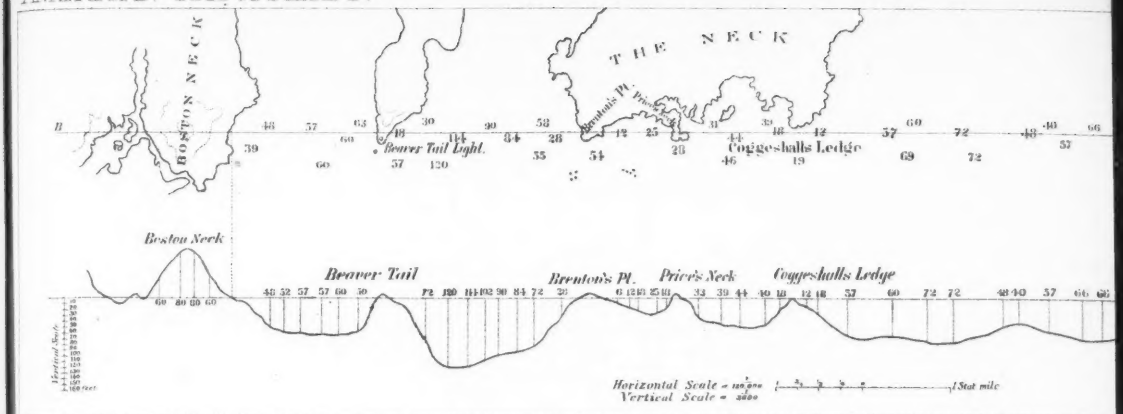
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Vertical Scale = 1/4000
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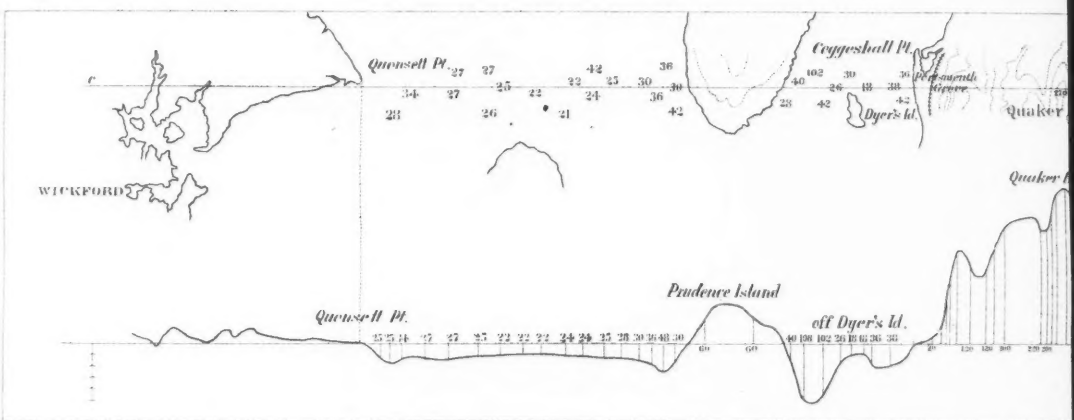
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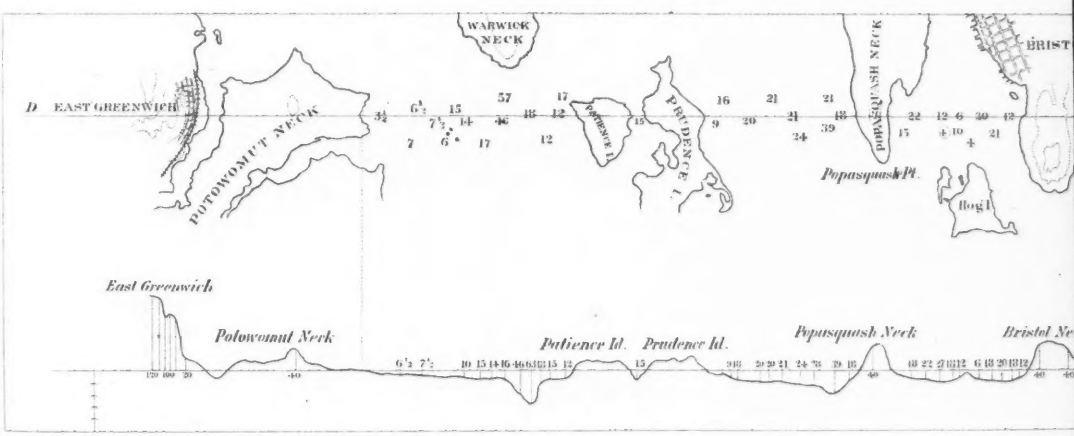
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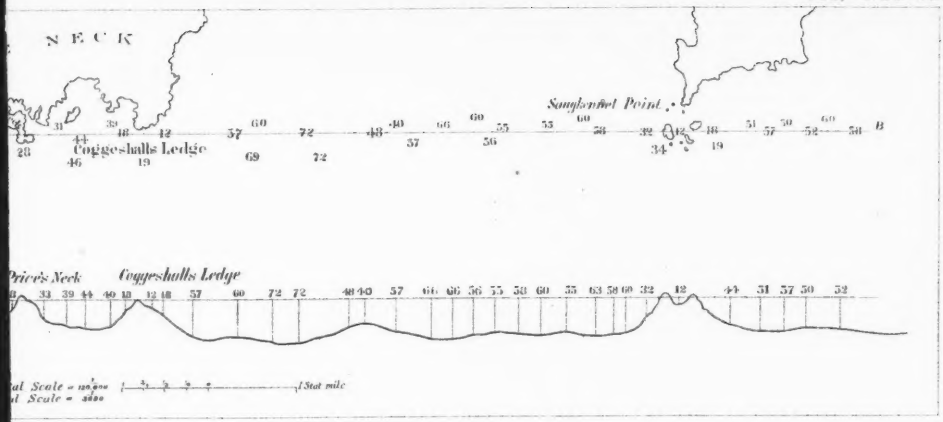
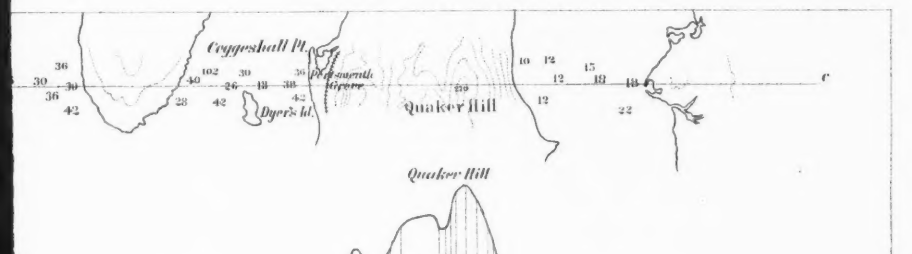
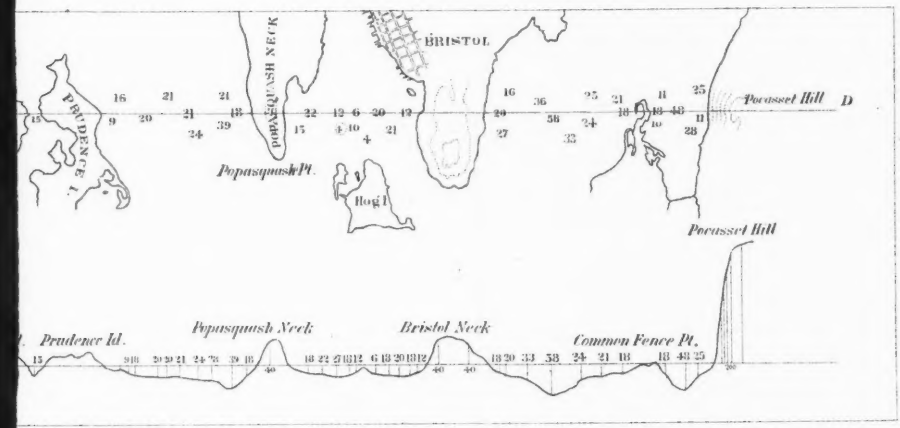
Patience Id.
Prudence Id.
Popasquash Neck







New Eng. Lith. Co. 104 Summer St. Boston.

men; there can be no doubt, however, that it was quite uniformly covered with boulders of varied size. The thousand miles or thereabouts of stone walls which are found upon the island attest the original abundance of these surface boulders. Most of the larger masses have been broken up in order to be used for building purposes, but there still exist several thousand erratics having a diameter of more than three feet which have been deported from the cultivated fields, or lie in the regions which are too barren for cultivation. If we examine the sections in the drift, visible at various points, and notice the comparative variety of large boulders, it seems no easy matter to account for these numerous surface erratics without resorting to some such agency as floating ice operating at the close of the principal glacial period. This method of explaining the abundance of surface erratics is much in favor with many European geologists. There are many arguments against it, however, which make it in the highest degree unlikely that it was the main agent. On the highest hills these boulders abound the most; now there is every reason to believe that there has been at the most not over forty feet of subsidence on this shore since the beginning of the glacial period; a depression so considerable as to have lowered the level of the highest hills of Aquidneck island beneath the level of the sea, must have left some recognizable evidence of its existence. There is, however, no evidence sufficient of such a change of level. We can more satisfactorily account for the facts by assuming that these boulders of large size owe their abundance to the following causes. The rock masses in the glacier which were furthest from the rock bed would receive the least attrition and therefore remain the largest in size; moreover, the continual wear to which the upper part of the drift bed has been subjected has served to wash away a large quantity of the mud, which forms a great part of the mass, leaving the pebbles of various sizes accumulated on the surface. These great boulders should, on account of their superior position, have been riven from their parent rock at a greater distance from their present position than the other which lies below them. It is a tolerably safe conclusion that the lowermost pebbles in the mass which were riven by the ice from the bed rock, would be the last additions to the accumulation. Each successive accumulation would lift the débris which was already in existence, so that while in the time immediately after its separation from its bed the

fragment of rock would be subjected to great wear, it might, by the subposition of fragments more recently sundered, be gradually lifted out of the level of greatest attrition into a level where it would be carried without much wear. When the rock over which the ice flowed yielded fragments more rapidly than they were ground up, the conditions would be the most favorable for distant transportation; when, on the other hand, if the grinding of the pebbles went on more rapidly than they were supplied, the chance of distant transportation would be small. This is well illustrated over the southern part of Aquidneck island. When the glacier swept over the friable slates, the drift drawn from the northern end of the island kept in security in the upper part of the débris; when it passed on to the southern part of the section where the hard felsites and argillites were encountered, the packing of soft shale was quickly ground up, and then the syenite pebbles were let down upon the bed rock and soon reduced to fine material. The result is that the region known as the neck, south of Newport, has a very thin coating of drift matter.

All the boulders, of whatever size they may be and at all positions in the débris, show evidences of abrasion which cannot be explained without supposing that they were derived from the floor of the ice mass, and passed through the inevitable jostling which must occur while they were at the bottom of the pack.

There can be no question but that the retreat of the ice across the section given in this island must have been, in a geological sense, very rapid. The evidence is clear that at one time it extended far to the seaward of the southern end of the island, so far indeed, that we can find no distinct evidence at this point of the great masses of débris, which must have been deposited at the outer border in the shape of a great terminal moraine. As the ice went back, if its retreat had been by successive steps, any of which brought the edge of the ice across this island, the result would have been the formation of a distinct terminal moraine. It cannot be said that such a mark would have been obliterated by subsequent changes, for the scratches which cover the uppermost pebbles of the drift would surely have been worn away by any erosion which could have had great effect on the contour of the surface. We are therefore justified in supposing that the glacial sheet retreated rapidly and steadily, when it passed from the surface of this part of our shore. It does not seem as if the term retreat

could be used with propriety for the simultaneous melting, which appears to have taken place in the whole section here. I am inclined to think that the ice stream, no longer fed with glacial matter and probably wasted by great rains and a strong heat, gradually came to rest, and then the same process of decay slowly separated the water from the stones, dropping them into their present resting places.

The evidence of a readvance of the glacial matter, not in the form of a great sheet, but in the shape of separate streams, is by no means so clear here as it is to the northward. If it came at all it only lasted for a short time, and produced no considerable effects.

I am inclined to think, however, that both the east and west channels gave passage to ice streams which returned to them long after the main sheet had disappeared. These local glaciers did not, however, reach farther south than the middle of the islands.

The scorings upon the surface of the rock tell the same thing in all parts of the island; the general course is from the north by west towards the south by east, but the individual differences are considerable. Many scratches depart widely from this range. In the rude jostle which took place at the point of contact of ice and rock, there would necessarily arise just such changes of direction as are indicated on the rock surfaces. There is also a certain accommodation of direction of slope to contour of surface before a stout boss of rock, where the flexible stream would turn a little for an easier escape.

The cutting of the scratches is not as deep as in more northern regions; there is nothing like the long deep drawn grooves of northern Vermont, or the region about the Great Lakes. This may be due as much to the prevailing weakness of the cutting tools, the pebbles, as to a difference in the energy of movement of their setting, the ice. On Price's neck, a singularly good specimen of glacial erosion, the scratches are quite distinct and the amount of surface which shows ice action unusually large. The point of greatest interest connected with this locality is found in the evidence it affords concerning the depression of this region since the time when these furrows were graven. If it had been for any considerable time beneath the sea it would have surely been changed from its present character. I believe that most, if not all of it, was beneath the sea at the time when the glacial sheet

passed away, but the reëlevation was so quick that even these faintly traced lines were not worn away. Equally strong is the evidence they give concerning the nearness of the time when they were made. Although the rock is not the most enduring, having a considerable tendency to shatter, they stand almost as fresh as the carvings on Egyptian stones.

In attempting to measure the influence of the glacial action upon this region the first question which arises is whether we are to refer the whole of the erosion which is clearly to be attributed to this agent to the action of the ice time which immediately preceded the present geological period, or must look upon it as having been in part the work of more ancient glacial periods. Besides the general argument that there is a great improbability in the supposition that there has been but one glacial period in the earth's history, we have other reasons for believing that glacial action has been a constantly recurring element in the successive geological stages. The hypothesis of Mr Croll, which up to the present time is the most satisfactory theory that has been presented to account for the coming of the ice time, makes the change depend entirely upon the alteration in the eccentricity of the earth's orbit, and the change in the place in that orbit when the northern and southern hemispheres get their winter and summer exposures to the sun. Whether these hypotheses prove well founded or not there can be no doubt that the past geological record shows us evidences enough of glacial action. Every great conglomerate is in itself such evidence; nothing but a glacial period is competent to produce or transport, into their present position, such masses of pebbles as make up the conglomerate which is found in the carboniferous rocks of this island. At almost every stage in the earth's history we have the same sort of accumulations. They abound in the primordial, are frequent in the palæozoic and mesozoic, and numerous in the tertiary. The Roxbury conglomerate found in the town of that name, and over a large part of the south and west shores of Massachusetts Bay, is so much like the modern drift, that at one point, where it is rather more incoherent than usual, the eye does not at first readily detect the difference between the glacial beds of the geological yesterday and the underlying conglomerate of the primordial time, so close is the physical connection of these beds which are separated by nearly the whole life-bearing section of the earth's crust.

The fact noticed in the paper on the Topography of Aquidneck Island (see Vol. vi, page 518), that the streams on this island have the arrangement natural to a local water system, shows that the channels which separate it from the main land had been excavated before the last glacial period. Now, as we have already seen, these channels have a shape which can only be accounted for by supposing that they were formed by the excavating power of ice. Therefore we are compelled to suppose that there was an ice action anterior to the glacial period which gave way to make our present time. If this be true of this section of our fiord zone, it is likely to be true for a large part of the topography of our shores which is dependent for its shape upon ice action. It would be imprudent to rest so large a determination upon such a small basis of observation; the question is, however, one which invites the consideration of all those who are interested in working out the history of our continent. Nor is the problem one of local interest alone; our whole conception of the conditions under which life has been developed and the successive sheets of strata laid down will be greatly modified by such a view of the past as would be forced upon us when we recognized glaciation as a constant factor in the evolution of the physical and vital history of our earth. We need some conceivable agent whereby the life of the past could have been subjected to ever varying conditions. If it has been driven in endless chase from equator to poles by the alternating changes of heat and cold, these variations would have succeeded each other with startling rapidity. This view is consonant with all we know of life in the past; it furthermore seems in the highest degree fitting that life, the product, the localization of solar energy, should find the spur that drove it onwards and upwards in the changes of its motive force.

ON THE CAUSE OF DETERIORATION IN SOME OF OUR NATIVE GRAPE-VINES, AND ONE OF THE PROBABLE REASONS WHY EUROPEAN VINES HAVE SO GENERALLY FAILED WITH US.

[Concluded from page 544.]

BY C. V. RILEY.

MEANS OF CONTAGION FROM ONE VINE TO ANOTHER.— The young lice, whether hatched upon the roots or in the galls, are quite active and crawl about for some time; and that they will spread from one vine to another, either under ground upon the roots or on the surface of the ground during the night, is highly probable. Such, however, cannot be the mode of spreading from one vineyard to another; for were it so, the malady could not possibly have assumed such proportions in so short a time as it has done abroad. One method of transport is upon the roots of seedlings and cuttings, but the insect cannot in this manner find its way to an old vineyard, and there must be still another means. Here we come to that part of the natural history of our louse which must assume the form of hypothesis until further observations shall be made. In this country the malady is general, but in France, where it is still spreading from one place to another, they have a good opportunity to watch its progress; and Planchon finds that it always commences at certain circumscribed points and spreads from these points in more or less regular circles. There is no way of accounting for these nuclei—these starting points in the centre of an old vineyard that never showed signs of the disease before, except on the hypothesis of the winged insect having flown there and started the colony.

We have already seen that certain individuals of the root-inhabiting type become winged. Why these individuals become winged while others never do, is, perhaps, not for us to understand. Signoret ventures the Lamarckian suggestion that the need of quitting roots that are already destroyed may be one reason, and the pupæ are certainly found more particularly on badly infested roots. All plant-lice multiply agamically during the summer months

while they are abundantly nourished, but towards winter when, by this mode of reproduction, and by the diminishing nutriment in the dying foliage, the lice become, so to speak, exhausted, then lo and behold winged males and females appear! Numerous other facts in insect life such as the production of drone bees solely from unfertilized eggs, etc., indicate that the winged male may be, in some way or other, connected with defective vitality; and Mr. Thomas Meehan, of the "Gardener's Monthly," has so frequently observed such to be the case with plants, that he considers it a law "that with a weakened vitality comes an increased power to bear male flowers."* But this throws no light on the production of winged females, and here, as in thousands of other instances, nature tells us plainly to be satisfied with the facts without the explanation.

Our winged female is a reality! What, then are her functions? In the breeding jars she invariably flies towards the greatest light, and her large compound eyes, and ample wings indicate that she was made for the light and the air. We have also seen that she is burdened with two or three eggs only, and my opinion is that after meeting her mate, her sole life duty is to fly off and consign her few eggs to some grape-vine or grape-bud, and that the lice hatching from these eggs constitute the first gall-producing mothers.

I am led to this opinion by the fact that about the middle of May, in looking for the galls, I always find but two or three to a vine, and generally but one to a leaf. These vernal galls, as one would expect from the greater vitality of the young from fertilized eggs, and the greater succulency of the leaves at that season are much larger than the ordinary summer form, and generally have a decided rosy tint on one side. Similar galls have also been found in France. Just as many other insects prefer certain species of plants, or even certain varieties of a species, so our winged *Phylloxera* shows her preference for the Clinton and its close allies. She occasionally deposits her eggs on other varieties, as I have found the large vernal galls on Concord, Hartford Prolific, etc., and it follows that she must do so where no *riparia* vines occur. But, except on the varieties of the latter species, the young lice hatching from her eggs do not seem to be capable of forming galls on the leaves, but make straightway for the roots.

* Proc. Am. Ass. Adv. Sci., 1869, p. 256.

Only in this manner can we account for the galls abounding so much more on some varieties than on others.

Some persons may wonder how a minute insect with such delicate wings, braced with so few simple veins, as those possessed by our *Phylloxera* can manage to fly through the air to any great distance; and those who have not witnessed them in flight are very apt to underrate their powers of volitation. There is a conical gall very common on the upper surface of the leaves of our Shell-bark hickories. This gall is made by a louse very closely allied to our Grape-leaf gall-louse and was named *Phylloxera caryæfoliæ* by Dr. Fitch.* This louse occurs abundantly in the winged form, and furnishes an excellent illustration of the power of the insects of this genus to fly. Let any one watch these winged gall-lice, as they issue, during some warm day in June, from the fimbriated mouth of their gall, and he will be struck with astonishment at the facility and power with which they fly off. They are no sooner out of the gall than the wings begin to vibrate so as to become invisible, and the insect suddenly darts away with wonderful force. They must likewise often be carried great distances by the wind.

Again, it would at first sight seem almost impossible for the female to deposit her loose eggs which have no viscous property, upon a swaying leaf, but this very feat is accomplished by another little louse of the same genus,† which may be found depositing its eggs all through the summer months, on the under side of the leaves of our young Post oaks.

Probable Reason why its Injuries are Greater in Europe than with us.—It is a well recognized fact among careful observers, that in the natural state there is a greater harmony between the fauna and flora of a country than in the more artificial state that civilized man induces by cultivation. Through a long series of ages, the species least able to contend in the struggle for life "go to the wall," until at last by a process of elimination, the balance is struck and we find the animal and plant world well adapted and adjusted to each other. For this reason the native

* Rep. III. § 166.

† This is a species of *Phylloxera*, which is yet undescribed but which M. J. Lichtenstein proposes to call *Ph. Rileyi*. It infests the leaves of our Post oak very much in the same manner as the European *Ph. quercus* infests their oaks—causing a similarly pale speckled appearance of the upper side of the leaves. It differs from all described species in the great length and prominence of the tubercles.

vines which now flourish in this country are those which have fought the long battle in the past and have best resisted the enemy. They are, in short, best adapted to the circumstances, and by their more vigorous nature resist the hypertrophy of the bark caused by the punctures of the lice, and form new bark under it. The European vines, on the contrary, are not only of a more highly improved and tender character, but have not been accustomed to the disease. They consequently succumb more readily, on the same principle that many diseases that are comparatively harmless among civilized nations, acquire greater virulency and play fearful havoc when introduced among savage or hitherto uncontaminated peoples.

There may be other reasons such as the different modes of culture and differences of soil; for in the French districts, so badly affected, the vines are either grown with a single stake or no stake at all, and their soil is generally much poorer than ours. In America, also, we know that there are several natural enemies of the louse, and these checks have, in all likelihood, never been imported into Europe with their prey. That the louse will in time find enemies and lose its acute power of doing harm even in Europe, is highly probable; and M. Planchon has already noticed that the infested vines in the latter invaded departments of Gard and Herault retained a comparatively greener color than in that of Vaucluse, first invaded. Such has been the history of the Hessian fly and a number of other insects imported into this country. These are the explanations I venture, and whether they be generally accepted or not, the facts remain.

Outward and more visible Effects of the Root Disease.—As long as the lice are confined to the more fibrous roots which, in a measure, are renewed each year, the vines show no decided outward signs of the malady, which may then be considered in its incipient stage. As they become multiplied and fasten on to the larger roots, their work becomes more visible in a sickly, yellowish appearance of the leaf; and a reduced growth of the vine is the result. As the roots waste away, these symptoms become more acute, and at this stage of the disease the lice have generally left, so that when the vine is about dying it is often difficult to find any trace of the cause of death. On the rotten roots little eight-legged mites are frequently met with, and they are

also to be found in the galls. They may always be distinguished from the true lice by their white, or dirty-white, color.

Practical Suggestions.—Last year, from the knowledge we then had of this insect in this country I recommended the destruction of the Clinton vine, where other and better varieties succeeded as well. This advice was given in order to get rid of the galls, and wherever it has been followed it has had the desired effect. It was given, however, under the impression that the lice would not attack the roots except where the leaves were covered with galls; whereas, in truth, the roots would appear to be less affected (at least during the growing season) where the leaf-galls are abundant than where they are scarce; while they may be absolutely ruined where no signs of galls exist. Consequently there is no longer any urgent need of, or good reasons for, destroying our Clinton vines. By doing so we may diminish the number of galls, but we can never exterminate the root-lice. Future experiments will no doubt show that good results will attend the grafting of such varieties as are known to be most seriously affected, on to the roots of less susceptible varieties.

The insect should be especially watched, as it is apt to be most troublesome, on poor, gravelly or clayey soils. In deep, rich soils I think there is less danger. In France it has been found to be less troublesome on sandy soils, and in my studies I have always noticed that minute, soft-bodied insects do poorly in sand.* The greater the growth of vine the greater the growth of root, and consequently vines that are trained on walls and which thus more nearly approach the wild state, or which are rendered vigorous by a rich soil, are least susceptible to the disease.

Remedies.— *Destruction of the Gall-Lice.*— From what we have seen, we may justly infer that this insect cannot of itself spread from one vineyard to another without going through the gall-producing phase: and a few galls on the leaves are, no doubt, invariably the first signs of its advent, by natural means, into a vineyard not previously attacked. By natural means, I mean without the aid of man's assistance, by which they are introduced from one place to another on the roots. If these galls, therefore,

* In examining the vine roots this fall in some parts of Northern Illinois, where sand formed a prominent portion of the soil, I found very few root-lice; except on *riparia* vines whose leaves had been covered with galls. Even on these the general healthfulness of the roots indicated that they had not been infested during the summer, and that the lice had all come from the last galls of the season.

could only be found and destroyed, it would be one way of effectually heading off the evil; and in a new vineyard a little vigilance in searching for these galls might save much subsequent loss and labor. I shall not treat here of the natural enemies of the louse, which are of such a nature that they cannot be practically controlled and increased.

Destruction of the Root-Lice.— I hope next spring to institute a series of experiments on the root-lice, with a view to the discovery of a practical remedy. It were to be desired that others having opportunity and occasion would do likewise. Here is an excellent chance for our different agricultural colleges, which have greater means and facilities than any one individual can possibly have. As a guide to such experiments, and to profit as much as possible by the experience of others, I will synopsize the results of trials in France. From these results, which I give below, we may learn that no reliable and cheap remedy, that will destroy all the lice after they have become numerous, has yet been discovered; and the best advice that can at present be given is to guard against the insect's introduction into new vineyards by carefully examining the roots before planting. If knots and lice are found upon them the latter may be destroyed by the same means used against the Apple root-lice — *i. e.*, by immersing the roots in hot soap-suds or tobacco-water.

Preparations of carbolic acid have, so far, given most satisfaction, and I have great hopes of benefit from the saponaceous compound prepared in this country by James Buchan & Co., of New York. This compound is not yet manufactured in France, where they have to use the pure acid or the crystals.

Carbolic acid added to water at the rate of one-half to one per cent. has been successfully employed, and M. Leenhardt, of Sorgues, has by its use, succeeded in keeping his vineyard alive and bearing, while all those around him are destroyed. He uses a heavy bar, thickened and pointed at the end, wherewith to make two or three holes, a foot or more deep, around the base of each vine. He then fills these holes with the liquid, which gradually permeates the soil in all directions. A good post-hole auger, such as we use in this country, would work more rapidly, with the advantage of compressing the earth less, but it would do more injury to the roots.

Oil of Cade.— This empyreumatical oil, which is common and

cheap in France, when dissolved in any alkali (the urine of cows being good enough) and applied in the same manner described above, has also given good results. A mixture composed of lime and sulphur boiled in water at the rate of about five pounds lime and five pounds sulphur to one gallon of water, and applied when hot, has been found good.

Alkalies seem to invigorate the vines, but do not affect the lice. They are also too costly. *Salt.*— Vines on lands strongly impregnated with salt have been found to resist the attacks of the lice. *Acids* generally are neutralized by the lime which most soils contain.

Sulphur has been thoroughly tried without any good results, either upon the leaf-lice or root-lice.

Sulphuretted hydrogen.— They have tried to pump this into the soil, but the pumps always break and no one would think of going to such trouble here.

Sulphate of Iron is of no account. *Sulphate of Copper* destroys the roots. Numerous other chemicals have been experimented with, but with very little or no success, and they are besides not applicable on a large scale.

Irrigation and *Submersion* have been pretty thoroughly tested, and it is doubtful, even where they can be employed, whether they have any other effect than that of invigorating the vines, as the lice are, many of them, found alive after a submergence of months. These methods must be considered conservatives rather than curatives.

Résumé of the Insect's History.—We have had in this country, from time immemorial, an insect attacking our native vines, either forming galls on the leaves or gall-like excrescences on the roots.* This insect is polymorphic, as many others of its family are known to be. It also exists in two types, the one, which may be termed *radicicola*, living on the roots, while the other, which may be termed *gallæcola*, dwells in galls on the leaves. The latter is found more especially on the Clinton and its allies, while the former is found on all varieties, but flourishes best on vines belonging to the *vinifera* species. The gall-inhabiting type was noticed and imperfectly described in 1856, but the root-inhabiting

* I have been able to trace them with absolute certainty as far back as 1845, for in the herbarium of Dr. Engelmann is a specimen of wild *riparia* gathered in this State in that year, the leaves of which are disfigured by the very same gall.

type, being less conspicuous, was unknown in this country till last year.

Such an insect is very readily transported from one country to another on grape roots, seedlings, etc., and just as our Apple, root-louse (*Eriosoma lanigera* Hausm.) was imported into Europe towards the close of the last century, so we find that our Grape-louse was similarly imported, in all probability within the last decade. The mode of transport will become all the more intelligible when I state that M. Signoret showed me, last July, the yet living progeny of some lice which he had placed in a tightly-corked glass tube the year before; and that he had managed to keep a few alive for study through the siege of Paris up to the time mentioned.

Nothing would be more natural than its introduction at Bordeaux, where M. Laliman has, for a number of years, been assiduous in the cultivation and trial of our different American vines. Or it might have been introduced at the nurseries of the Audebert Bros., near Tarascon,* where all sorts of American plants have been cultivated; and, if I mistake not, M. Planchon with commendable zeal, has so thoroughly sifted the history of the subject in France that he can trace the first invasion, with tolerable certainty, to a point near this place, Tarascon. It doubtless existed in France a few years before its injuries attracted attention, and the first notice of its work was made in the vineyard of M. de Penarvan, at Ville-neuve-les-Avignon, in 1863. The scourge soon increased and spread, and in 1868 and 1869 acquired such dimensions as thoroughly to alarm the great grape-growing districts of beautiful France. At first all sorts of hypotheses were put forth as to its cause. Some book-worms even thought they had found in this root-louse the *Phtheir* of the ancient Greeks, but the intelligent labors of M. Planchon dispelled all such illusions, and and proved that the *Phtheir* of the ancients was a true bark-louse (*Dactylopius longispinus* Targ.) of a totally different nature and still existing in the Crimea.†

*M. Laliman, in the essay already mentioned (p. 63), shows that this nursery has not existed for nearly fifteen years; but this fact does not preclude the possibility of the louse having been first introduced there. It would only indicate, if the spread of the disease can be traced from that point, that it existed in France, without attracting attention, at an earlier epoch than is generally supposed.

†See an Essay entitled La Phthiriose ou Pédiculaire de la Vigne chez les Anciens. Bulletin de la Soc. des Agr. de France, July, 1870.

In this manner our root-louse was known and studied in a foreign land before its presence was even suspected in this — its native country.

Conclusion — No Need of Unnecessary Alarm. — Knowledge of the facts I have here brought forth need not alarm the grape-grower any more than correct knowledge of some indisposition, hitherto incomprehensive and consequently uncured, should alarm the human patient. It was only a few years ago that our eyes were opened to the true character of the entozoon known as *Trichina spiralis*, and there can be little doubt but that previous to our knowledge of this parasite many a death occasioned by it was attributed to other unknown causes. It may not be more easy to cure the disease now than it was formerly, but we are, by understanding its nature, enabled easily to guard against and prevent it. "Full knowledge of the truth," says Helmholz, "always brings with it the cure for the damage which imperfect knowledge may occasion." The Phylloxera has always existed on our vines, and those varieties which in the past have best withstood its attacks will be very likely to do so in the future. The presence of a few lice on such varieties need cause no fear, for the idea of ever entirely exterminating such an insect from the country must be perfectly utopian, and all we can do is to watch and more particularly care for those varieties that most easily succumb. In the future, the vineyardist will be enabled, by the revelations here made, to trace to a definite cause many a failure which has hitherto been wrapped in conjecture and mystery.

In thus calling the attention of the grape growing community to this interesting little insect, which is sapping the roots of their vines, my intention is to do good and not cause unnecessary consternation. Let me hope that others may be induced to study the microscopic plague and thus not only assist to fill the gaps yet occurring in its natural history, but help us to become better masters of it. Only those who have witnessed the fearful havoc it has made abroad, where in three years it caused a loss of 25,000,000 francs in the single department of Vaucluse, France, can fully appreciate its importance and its power, under favorable circumstances, to do harm.

I must remind those who live outside of Missouri, that my observations in this country have been confined to different parts of this State and apply more especially to this portion of the

Mississippi Valley. The insect occurs, however, very generally over the country east of the Mississippi river, even into Canada; and there are strong indications that it produces similarly injurious effects elsewhere. To give a single example: — according to the records, most of the vineyards on Staten Island which were flourishing in 1861, and which were composed principally of Catawba, had failed in 1866, and Mr. G. E. Meissner, of Bushberg, who then owned a vineyard on that island, informs me that he had noticed the nodosities, and that the roots of the dying vines had wasted away.* I cannot conclude without publicly expressing my indebtedness to Messrs. Lichtenstein and Planchon, of Montpellier, France, for the cordial and generous manner in which they gave me every facility for studying the insect there, and witnessing experiments in the field.

REVIEWS AND BOOK NOTICES.

Recent Discoveries in Ornithotomy. — The authors' courtesy places two very notable papers on our table. I. Prof. Morse's embryological studies† have furnished one of the most important contributions ever offered in this country to our knowledge of the structure and development of birds. His entirely original researches, conducted with scrupulous care, in the most candid spirit, not only confirm the late determinations of Gegenbaur and other European anatomists, but take a long step beyond. We have from time to time been apprised of the author's progress in the investigation, and since its close have given the subject the closest scrutiny. In stating the points we believe that the author has established, we must also indicate those that we hold to be still questionable.

Prof. Morse finds four tarsal bones. Three of these have been very generally recognized of late, although rejected or at most not

* Since the above was written, I have listened to an essay on Grapes, by Mr. P. Manny, of Freeport, Stephenson county, Illinois. In this essay, which was read before the Illinois State Horticultural Society, the writer states that his Delaware, Iowa and Salem vines lose their lower roots. He attributes this loss of roots to the tenacity of the soil (though more likely owing to unseen root-lice) and has remedied it in a measure by grafting on Clinton roots.

† *On the Carpus and Tarsus of Birds.* By Edward S. Morse, Ph. D. Ann. Lyc. Nat. Hist., N. Y., X, 1872.

fully accepted, by such an eminent authority, for instance, as Owen, whom the author very properly arraigns for his unphilosophic handling of the carpal-tarsal homologies. They are two near bones, anchylosing together, and with the tibia, serving to form the characteristic avian bitrochlear malleoli; and one far bone which unites with the metatarsals. For these Prof. Morse accepts the determinations implied in Gegenbaur's nomenclature, calling them *tibiale*, *fibulare* and *centrale* (astragalus, calcaneum and naviculare). Many interesting details of the form, time and mode of fusion, etc., in different species are given. The fourth bone is a "new" one, the recognition of which as an integral element of the avian tarsus we owe to Prof. Morse. Authors have described a so-called "process of the astragalus" in certain birds, and it has been stated that the astragalus of higher vertebrates represents the tibiale and the intermedium (of reptiles), connate; but it remained for Prof. Morse to determine that a certain "pre-tibial" bone of a species of heron, noted in Prof. Wyman's manuscript, now published, as finally uniting the astragalus to form a "process" identical with that described by Huxley, is a distinct tarsal element which, for anything appearing to the contrary, must be identified with the reptilian *intermedium*. We consider this view entirely reasonable — in fact, we know of no other warrantable conclusion from our present data, and we are thus prepared to adopt all of Prof. Morse's views respecting the composition of the avian tarsus, without reserve or qualification.

If we must think that he has not been equally successful in determining the structure of the carpus, we are at least prepared to show cause for our lacking faith in some of his conclusions. Respecting this segment the author writes: "In the fore limb or wing there are at least four carpal bones, two in the proximal series, and two in the distal series. *When more than four carpals occur, as in the*" etc. The two of the near series are those that persist free in the adult carpus, well known as "scaphoid" (or "scapholunar") and "cuneiform," or, in better nomenclature, *radiale* and *ulnare*. The two far bones come of Prof. Morse's resolution of the so-called "epiphysis of the metacarpal into two carpal elements, one capping the mid-metacarpal, the other the annularis. Since the metacarpals of higher vertebrates, excepting that of the pollex, are well known to lack a proximal epiphysis, the part in question was early determined to belong to the carpus,

though it has been generally recognized as only a single bone. Prof. Morse's figures show plainly that there are always *two* ossific centres, and that the ossicles are often as distinct from each other as are the metacarpals they respectively cap. This important point may be considered as established. The author is uncertain of the homologies of these two far bones, querying whether that one capping the mid-metacarpal be intermedium and centrale connate, or *carpale iii* ("magnum"); but he provisionally holds it as *carpale iii*, and the other as *carpale iv*, as marked in all his figures.

The sentence we have italicized above will be met with surprise if not with suspicion, and we cannot believe, upon the data furnished, that more than four carpals occur. Indeed, the author himself explicitly records his uncertainty respecting the "extra" carpals mentioned as *apparently* present in Tyrannus and Dendrœca. The piece marked "*c*" in figure 47 (Tyrannus) is doubtless "the result of accidental pressure" which separated it from *carpale iii*, while that marked "2" in figure 48 (same bird) seems unquestionably pressed apart from *radiale;* in each case we would emphasize the author's words: "it is safe to reject its occurrence for the present." In the case of Dendrœca, we think that the difficulty of the supposed extra carpal can be satisfactorily explained. Prof. Morse has been led to believe that "the *ulnare* may unite with the ulna," because he several times observed a close mutual appression of the two bones, and failed to find an *ulnare* at all in two instances. But in figure 43, where no ulnare is represented, we are satisfied that the missing bone simply escaped the field of the microscope; while in figure 44, the bone marked "*i*" and supposed to be an additional carpal on the radial side is, in our judgment, the ulnare itself accidentally displaced to the right. In this last figure, it will be observed, the author queries the ulnare as having anchylosed with the ulna; but unless we are altogether misinformed, no such anchylosis ever occurs. In all birds, so far as we know, the ulnare persists free, and gives an insertion to the *flexor metacarpi ulnaris* (just as is incidentally shown in fig. 34). Accounting as above for the respective disagreements of figures 43, 44, 47 and 48 with the others, all the figures show clearly the four carpal bones that the author may justly claim to have established.

Respecting the carpal-tarsal homologies, of which the author

does not specially treat, we should say that, while the antitypy of *tibiale* and *ulnare* and of *fibulare* and *radiale* appears unquestionable, observations are wanted to show the relations of the remaining tarsal bones with those of the distal carpal series; for if Prof. Morse correctly identifies the latter as *carpale iii* and *iv*, these do not correspond with those of the tarsus determined as *intermedium* and *centrale*.

This remarkably able paper is illustrated with two colored plates containing forty-eight figures from the author's accomplished pencil, representing exactly what he saw. We trust that Prof. Morse will continue his admirable embryological studies, and show us more new things. Ornithology has plentiful need of workers of his stamp.

II. Although we only undertake to give a complete record of *American* papers relating to ornithology, we shall have frequent occasion to notice foreign publications of general interest or special importance. Prof. Reinhardt* makes an important announcement respecting "l'os crochu" (*os uncinatum*) observed by Magnus in Phœnicophæus, and which, he states, is the same as the *ossiculum lacrymo-palatinum* described by Brandt in Tachypetes and various *Procellariidæ*. He further states that this bone is highly characteristic of the *Musophagidæ*, having found it in all the genera and nearly half the species of Turacous. He describes its peculiarities in detail. Among the Cuculidæ, besides Phœnicophæus, he has seen it in Zanclostomus, Rhinortha and Eudynamys, whilst it is wanting in Centropus and several American forms. In the *Trogonidæ*, a species has the uncinate bone "represented by a slender falciform apophysis of the descending ramus of the lachrymal, bending towards the palate." Tachypetes remains the only totipalmate bird in which the ossicle is known to occur — which is interesting, seeing that the frigate alone of *Steganopodes* is petrel-like in superficial aspects. The bone appears to be as characteristic of the *Procellariidæ* as of the Turacous, occurring in nearly all of the genera of that family, though among the "stormy petrels" (Thalassidroma Auct.) it is merely represented by a ligament; but what, if any, relationship it may indicate does not appear. Prof. Reinhardt agrees with Brandt that the function of the bone relates rather to the olfactory than to the visual apparatus.

* *Om en hidtil ukjendt Knogle i Hovedskallen hos Turakoerne* (Musophagides), etc. Af J. Reinhardt. Vidensk. Meddel. f. d. Naturh. Foren. Kjöbenhavn. 1871.

The author takes the opportunity of correcting Huxley's hasty generalization respecting the absence of basipterygoids in *Procellariidæ* – an error which, however, had been already exposed.* Although these processes are absent in Diomedea and rudimentary in "Thalassidroma," they are well developed in other genera; and the fact is, that basipterygoids are highly characteristic of the *Procellariidæ*. — ELLIOTT COUES.

GUIDE TO THE STUDY OF INSECTS.† — A third and revised edition of this work has appeared with a few changes and additions of considerable importance. The author calls attention to the change in his views as to the number of segments in the head of six-footed insects; there being four instead of seven. A brief account of the recent discovery of Parthenogenesis in the pupa of a Chironomus is given; and also of wax-secreting glands lately found in wax-producing insects, *i. e.*, the bees, aphides and bark lice (*Coccidæ*). Moreover a correct figure of the caterpillar and chrysalis of *Melitæa Harrisii*, takes the place of the incorrect representation in the first two editions, while a short notice of the worm-like mites, *Linguatulina*, is inserted.

The Appendix consists of illustrated notices of the early stages of certain ichneumon parasites; of the embryonal membranes of insects; of the development of the louse: the mode of formation of the wings of insects; of Paolia, a remarkable fossil carboniferous insect and of abdominal sense organs of certain insects, while sketches of Schiödte's new classification of the Hemiptera, and Thorell's arrangement of the spiders are presented. Two plates are added, illustrating injurious and beneficial insects. Two other steel plates, one of lice, the other of Thysanura are added, and several new species of insects referred to.

THE DESMIDS.‡ Microscopists will welcome this beautiful memoir on the desmids of Sweden. It will be invaluable to American observers, as there is so much similarity between the

* By T. HALE STREETS. Proc. Phila. Acad., 1870, 85.

† Guide to the Study of Insects, and a Treatise on those injurious and beneficial to Crops: for the use of Colleges, Farm-Schools, and Agriculturists. By A. S. Packard, Jr., M.D. With 15 plates and 670 woodcuts. Third edition. Salem, 1872. 8vo. pp. 715. Price reduced to $5.00.

‡ De Desmidiaceis, quæ in Suecia inventæ sunt. Observationes criticæ. Auctore P. M. Lundell. Cum tabulis V. pp. 100. Nova Acta of the Royal Society of Sciences of Upsala, 4to, 1871.

desmid floras of the two countries. It is written entirely in Latin. Five excellent plates illustrate the paper.

BOTANY.

LEMNA POLYRRHIZA.— While botanizing last week on the Platte river in Nebraska, I found, near Fremont, large quantities of *Lemna polyrrhiza*, and upon close inspection to my great joy found many specimens in bloom. As this is rarely found in bloom, it may perhaps be deemed worthy of record. I secured specimens which I shall be glad to distribute. I may as well mention also that in the same trip I found *Euphorbia marquiata* in great abundance in western and northwestern Iowa, though Prof. Gray puts it as far west as the "plains of Kansas and Nebraska."— C. E. BESSEY, *Iowa State Agricultural Coll.*

NEW BOTANICAL WORKS. — A new part of Bentham and Hooker's "Genera Plantarum" is in the printer's hands, as we learn from Trimen's "Journal of Botany," and is expected to be out by the end of October. It will comprise Rubiacæ, Compositæ, and the intervening orders.

Mr. M. C. Cook's new journal "Grevillea," devoted to Cryptogamic Botany and its literature, contains much that will interest American botanists. The August number continues Mr. C. A. Peck's "New York Fungi," and announces that a series of papers on North American Fungi, by Rev. J. M. Berkeley will be begun in the September number.

We have received the first number of the "Transactions of the Imperial Botanic Garden at St. Petersburg," an octavo of 164 pages, printed partly in Russian and partly in Latin.

ZOOLOGY.

TORNARIA, THE YOUNG STAGE OF BALANOGLOSSUS. — The development of Tornaria has at last been solved. As is well known Müller, Krohn, Fitz Müller and myself have considered it a starfish embryo. The analogy between a Brachiolaria and Tornaria seems complete and no one questioned the position of the latter till Metznikoff in 1870 was fortunate enough to raise Tornaria to a later phase of development — to his astonishment it changed into an annelid. Of course, in view of the affinities (first sug-

gested by Huxley) of the worms and echinoderms, it was most important that Metznikoff's observations should be repeated, and if possible the genus of annelids, of which Tornaria was the young, accurately ascertained. The annelid raised by Metznikoff was most peculiar and, in absence of other evidence, he suggested the possibility of its being a young Balanoglossus. I have been able this summer to raise Tornaria and to obtain young annelids somewhat older than those observed by Metznikoff, tracing at the same time the development of the branchiæ as diverticula from the œsophagus, and also to find the young annelid of Tornaria a species of Balanoglossus (of which the adult is quite common at low water mark at Newport and at Beverly, Mass.), but slightly older than those raised directly from the Tornaria stage. The details of this interesting embryology will shortly be published. — A. Agassiz.

The Peculiar Coloration of Fishes, mentioned by your correspondent Richard Bliss, is, it appears to me, susceptible of easy explanation. The pigment-cells containing the brilliant crimson, adorning the skins of *cyprinidæ* and other fishes, are readily opened and closed under excitement and other influences. When brilliant *Cyprinid* are confined in aquaria they speedily lose their color by the closing of the cells, but it may be readily restored by scratching the surface with the point of an instrument, which reopens the cells. Even specimens freshly placed in weak alcohol may sometimes be made to display bright color by the same process. When the alcohol is strong, it may so contract the surrounding tissue as to expose the contents of the cells, as in the case observed by Mr. Bliss. — Edw. D. Cope.

A New Species of Passerculus from Eastern Massachusetts. In December, 1868, I took a sparrow at Ipswich which was then supposed to be *Centronyx Bairdii*. In the autumn of 1870, I took two more of the same species, also at Ipswich; but upon visiting the Smithsonian Institution this spring and comparing these specimens with the original *C. Bairdii*, I have come to the conclusion that they are specifically distinct. They are closely allied to the savanna sparrow and evidently belong to the same genus; I therefore propose to name the Massachusetts bird *Passerculus princeps*, the large barren ground sparrow. The *Centronyx Bairdii* should also, I think, be referred to the genus Passerculus, for I can

see no good generic character by which it can be separated. A description and figure of this new Passerculus, will be found in the "Naturalists' Guide" (page 112) under the name of *Centronyx Bairdii*, with a history of the capture of the first specimen and also an account of how this name came to be applied to it. It will likewise be understood that the name of *Centronyx Bairdii*, given in a notice in the May number of the NATURALIST (page 307) by Mr. Brewster, should read *Passerculus princeps*.

The *Thalasseus Havelii*, mentioned by Mr. Brewster in the same article, should also read *Sterna Forsterii*, for I have become convinced by carefully studying a large number of specimens that the *Thalasseus Havelii* = *Sterna Havelii* of authors, is the young of *Sterna Forsterii*. — C. J. MAYNARD.

GEOLOGY.

OIL CREEK PETROLEUM KNOWN IN THE LAST CENTURY.—At a meeting of the California Academy of Sciences, July 15th, Mr. R. E. C. Stearns presented a communication, embodying the following extract from the Massachusetts Magazine published in the year 1789, Vol. i, p. 416, showing that the existence of petroleum in Pennsylvania was known at that period:

"In the northern part of Pennsylvania, there is a creek called Oil Creek, which empties into the Alleghany river, It issues from a spring, on the top of which floats an oil similar to that called Barbadoes tar; and from which one man may gather several gallons in a day. The troops sent to guard the western posts, halted at this spring, collected some of the oil and bathed their joints with it. This gave them great relief for the rheumatism with which they were afflicted. The water of which the troops drank freely operated as a gentle purge."

MICROSCOPY.

SUCCESSIVE POLARIZATION OF LIGHT.—This curious scientific procedure has been accomplished in connection with the ordinary polarizing microscope, by Mr. J. W. Stephenson. In the selenite fitting of the polarizer, between the polarizing prism and the stage, he places, mounted so as to have a rotating movement of its own, a truncated glass prism having its broadest face silvered by the sugar of milk process, and its other faces so situated that light, polarized by passing through the Nicol's prism in a direction

parallel to the silvered surface, is refracted on entering the prism, meets the silvered surface at an angle of 72° and is, after reflection, refracted on leaving the prism into a course parallel to that at which it entered. Some of the curious effects produced are mentioned in the Transactions of the Royal Microscopical Society.

A Double Erecting Binocular. — In the Report of the Croydon Microscopical Club, it is stated that at its last annual meeting there was exhibited an erecting binocular on Mr. Stephenson's plan, made double so that two persons could examine the object at the same time.

Angular Aperture of the Eye. — It having been carelessly assumed that in order to obtain a perfectly natural effect, the lenses of a photographic camera must be reduced to a linear aperture $\frac{2}{10}$ in., the average diameter of the pupil of the human eye, and that a large aperture gives objects differently from ordinary vision, Mr. G. S. Cundell observes, in the "Monthly Microscopical Journal," that the ocular focus is only about $\frac{8}{10}$ in., and therefore only four times its linear aperture. A corresponding size of camera lens having 12 inch focus would be 3 inches, not $\frac{2}{10}$ inch. Such a lens, so far as aperture is concerned, would be as free from aberrations and "distortion" as the human eye. For the sake of stereoscopic effect he would use a lens of $2\frac{1}{2}$ inches diameter, that corresponding with the average distance apart of the human eyes, and reproducing the object, a head for instance, as we see it in nature, without the hard, cutting lines of monocular vision, which all painters deprecate and avoid.

NOTES.

We take pleasure in calling the attention of our readers to the publication, in another portion of this number, of the several memorials made to the Commissioners of the Central Park in New York, relative to Dr. Hawkins, and the restoration of his work so wantonly destroyed by order of Mr. Hilton, who, we understand, is no longer in charge of the works of art and nature which he evidently did not have the education fully to appreciate. As these memorials are the expression of the most cultivated and influential class of citizens of New York, and can but meet with a warm response throughout the country, we trust that the present Board of Commissioners will fully realize the importance of restoring

Dr. Hawkins' work and of allowing him to extend the very instructive series which he had planned. Not only does it devolve on the Commissioners to render justice to Dr. Hawkins, but also, by the restoration of the work and the reinstatement of Dr. Hawkins, to remove a blot from the fair fame of American science.

It seems as if the interference with the work of scientific men and their snubbing by the "Heads of Departments" had been epidemic of late, for not only have we to regret the treatment of Dr. Hawkins and Dr. Parry in this country, but also the equally outrageous treatment of Dr. Hooker by the Head Commissioner of the Kew Gardens in England, as lately made public in "Nature." Are we really going back to the early days of science, when a scientific man was looked upon as a subject for the mad house, or is it owing to the fact that "brains are scarce in high places?"

We take this occasion to correct an impression which we find is held by some of our readers relative to the American Museum of Natural History, which is supposed by many persons not acquainted with the complex system of the Central Park, to be under the same government as that which Mr. Hilton represented, and that its officers were in a measure responsible for the outrage perpetrated. Such is not the case, and we should feel great regret if any remarks of ours had been construed to the disadvantage of that institution, which we believe to be doing good work and worthy of all assistance from scientists throughout the country.

[Wishing to give the address by Prof. Gray in this number we have been obliged to postpone a large amount of Miscellaneous matter which was in type.—EDS.]

ANSWERS TO CORRESPONDENTS.

C. B. G., New York.—The spider is *Epeira spinea* of Hentz.

BOOKS RECEIVED.

Preliminary Description of new Tertiary Mammals. Part iii. By Prof. O. C. Marsh. Published Aug. 13, 1872. (Received, Aug. 14, 1872.) 8vo. pp. 9. From the American Journal Sciences and Arts. Sept. 1872.

Note on Tinoceras anceps. By O. C. Marsh. Published, August 24, 1872. (Received Aug. 26th.) From Amer. Jour. Sci. Arts, Oct. 1872.

Proceedings Academy Natural Sciences, Philadelphia. 8vo. pp. 73-88. July 16, 1872. pp. 105-120. (Received July 25th.)

Transactions of the Imperial Botanic Garden of St. Petersburg. Vol. i, No. 1. 8vo.

Grevillea, a monthly Record of Cryptogamic Botany. August, Vol. i. No. 2. London. Edited by M. C. Cooke. 8vo. pp. 16.

Notice of some New Tertiary and Post-Tertiary Birds. By Prof. O. C. Marsh. Published Aug. 30, 1872. (Received Aug. 31.) From the Amer. Jour. Sc. Arts. Oct. 1872. 8vo. pp. 8.

Preliminary Description of New Tertiary Reptiles. By Prof. O. C. Marsh. Part i. Published Sept. 6, 1872. (Received Sept. 9, 1872.) From Amer. Jour. Sci. Arts. Oct. 1872. 8vo. pp. 7.

Preliminary Descriptions of New Tertiary Mammals. Part iv. Published August 17th, 1872. From the American Journal of Science. Sept. 1872.

Die Grundlagen des Vogelschutz gesetzes. Von G. R. v. Frauenfeld. Wien. 1871. 8vo. pp. 12.

Die Wirbelfauna Niederosterreichs. Von G. R. v. Frauenfeld. Wien. 1871. 8vo. pp. 16.

Der Vogelschutz. Von G. R. v. Frauenfeld. Wien. 1871. 8vo. pp. 48.